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Dr. J. H. JEANS, PRESIDENT, in the Chair.

Harold Lee Alden, M.S., Ph.D., Leander McCormick Observatory, University, Charlottesville, Virginia, U.S.A. ;
Joseph Canon Boyce, Trinity College, Cambridge ;
Ernest Reginald Bryan, A.I.C.A, 145 Cromwell Road, S. Kensington, London, S.W. 7 ;
Rev. Evan Cruickshank, M.A., Drumtochty, Kincardineshire, N.B. ;
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Thomas Kelsey, Morton, Gainsborough, Lincs ;
Alexander Lorimer Kennedy, B.E., Chief Assistant, Adelaide Observatory, S. Australia ; and
Raymond Streatfeild, 42 Ovington Square, London, S.W. 3,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Alampallam Aiyapattai Narayana Aiyar, Kodaikanal Observatory, South India (proposed by T. Royds) ;
Jaroslav César, D.Sc. (Prague), Secretary, Czechoslovak Legation, London, and 42 Ambrose Avenue, Golders Green, London, N.W. 11 (proposed by Harold Jeffreys) ;
Angarai Seshiah Ganesan, 44 Lillie Road, West Brompton, London, S.W. 6 (proposed by A. Fowler) ;
Robert Edward Taylor, 18 Avon Road, Keynsham, near Bristol (proposed by E. Fawcett White) ; and
J. F. B. Wardale, B.A., 35 St. George's Square, Westminster, S.W. 1 (proposed by Mary Proctor).

The following were proposed by the Council as Associates of the Society :—

Julius Bauschinger, Director of the Observatory, Leipzig, Germany ; and

William S. Eichelberger, Naval Observatory, Washington, U.S.A.

Fifty-five presents were announced as having been received since the last meeting, including, amongst others :—

Portrait of Sir George Airy (presented by Mrs. Pocock) ;

White Ensign flown on Transit of Venus Expedition to Kerguelen, 1874 (presented by Mrs. Pocock) ;

Louvain, University Laboratory of Astronomy and Geodesy, Publications, Vol. i., Nos. 1–13.

Tacubaya Observatory, Carte photographique du Ciel, zones $-15°$ and $-11°$ (7 charts).

On a Theorem of v. Zeipel on Radiative Equilibrium.

By J. H. Jeans.

1. The very surprising theorem recently published by Professor v. Zeipel,* and discussed by Eddington,† calls for examination on account both of its mathematical unexpectedness and of the revolutionary astronomical consequences to which it leads.

Professor v. Zeipel deduces his theorem from equations which are in effect identical with the equations originally given by Eddington,‡ which I have shown § to involve errors of the order of $4\pi\epsilon$, the rate of generation of energy per unit volume in a star's interior. The theorem claims to evaluate ϵ, but clearly is liable to errors of the order of magnitude of ϵ itself. These errors seem to me to invalidate the whole theorem and the astronomical consequences deduced from it by Eddington.

Professor v. Zeipel states his main theorem in the form

$$4\pi\epsilon = K\left(1 - \frac{\omega^2}{2\pi G\rho}\right)$$

where K is a constant. For our present purpose it is sufficient to consider the simple case of a star rotating so slowly that $\omega^2/2\pi G\rho$ may be neglected. The formula reduces to $4\pi\epsilon = K$, which would require that the generation of energy per unit mass should be constant throughout a star.

* *M.N.*, **84**, 665.
† *Observatory*, March 1925, p. 73.
‡ *M.N.*, **77**, 16; 596.
§ *M.N.*, **78**, 28.

2. If the intensity I of radiant energy flowing in any direction inside a star is expanded in the form

$$I=A+BP_1+CP_2+DP_3+EP_4+FP_5+\ldots \quad . \quad . \quad . \quad (1)$$

then I have shown * that the following equations are rigorously true, k being written for the coefficient of opacity,

$$\frac{1}{3}\frac{dB}{dx}=\epsilon\rho \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\frac{dA}{dx}+\frac{2}{5}\frac{dC}{dx}=-k\rho B \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{2}{3}\frac{dB}{dx}+\frac{3}{7}\frac{dD}{dx}=-k\rho C \quad . \quad . \quad . \quad . \quad (4)$$

$$\frac{3}{5}\frac{dC}{dx}+\frac{4}{9}\frac{dE}{dx}=-k\rho D \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{4}{7}\frac{dD}{dx}+\frac{5}{11}\frac{dF}{dx}=-k\rho E \quad . \quad . \quad . \quad . \quad (6)$$

etc.

If C and D both vanish, equation (5) shows that E must vanish, whence F, G, . . . all vanish, and similarly if any other consecutive pair of coefficients vanish. The most general solution will be one in which the first n coefficients exist and all the others are zero.

3. The solution $n=1$ in which A alone exists represents a mass of gas in isothermal equilibrium, and is of no astronomical interest.

The next solution in order of simplicity ($n=2$) is one in which A and B alone exist, while C=D= . . .=0. This is the solution used by Eddington. On putting C=D= . . .=0, equations (2) and (3) reduce to

$$\frac{1}{3}\frac{dB}{dx}=\epsilon\rho \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\frac{dA}{dx}=-k\rho B \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The total radiant energy per unit volume is $4\pi A/c$, so that we may put

$$A=\mu T^4 \quad . \quad . \quad . \quad . \quad . \quad (9)$$

On eliminating A and B from equations (7), (8) and (9), we find

$$\epsilon=-\frac{1}{3\rho}\frac{d}{dx}\left(\frac{\mu}{k\rho}\frac{dT^4}{dx}\right) \quad . \quad . \quad . \quad . \quad (10)$$

which is equivalent (except for terms arising from the curvature of the

* *M.N.*, **78**, 29 and 30.

star) to an equation, first given by Eddington,* which v. Zeipel† uses as the basis of his discussion, although he derives it in a different manner.

If we put C and D equal to zero in equations (2) and (3), we must do the same in equations (4), (5), (6), etc. Doing this we find at once, from equations (2) and (4), that

$$\epsilon=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This equation expresses a wider and more general conclusion than v. Zeipel's formula for ϵ. The proper interpretation of equation (11), however, is not that ϵ must be zero for every star, but that our simplified solution (C=D=. . .=0) is inapplicable except to the case of a star in which $\epsilon=0$.

4. The solution which comes next in order of simplicity ($n=3$) is obtained by retaining C, but taking D=E=F=. . .=0. Equation (5) now shows that C must be constant, while equation (4), reducing to

$$\frac{2}{3}\frac{dB}{dx}=-k\rho C,$$

gives, in conjunction with equation (2),

$$\epsilon=-\tfrac{1}{2}kC \quad . \quad . \quad . \quad . \quad . \quad (12)$$

showing that this solution is appropriate to the case in which ϵ/k is constant throughout the star.

5. Next comes the solution ($n=4$) obtained by putting E, F, . . . etc., equal to zero, but retaining D. Equation (6) shows that D must be constant, while equation (5) gives

$$C=C_0-\tfrac{5}{3}D\sigma,$$

where σ measures the optical thickness of the matter, being defined by

$$d\sigma=k\rho dx.$$

Equations (2) and (4) now give

$$\epsilon=\frac{1}{3\rho}\frac{dB}{dx}=-\tfrac{1}{2}kC=-\tfrac{1}{2}k(C_0-\tfrac{5}{3}D\sigma) \quad . \quad . \quad . \quad (13)$$

showing that the present solution is appropriate to the case in which ϵ/k changes uniformly with the optical thickness.

6. We can proceed to higher degrees of complexity, the general solution in which n coefficients are retained being appropriate to a star in which ϵ/k is an algebraic function of σ of degree $n-3$. For an actual star, if we wish to avoid errors of the order of ϵ, we must use the full solution ($n=\infty$) in which all the coefficients are taken into account. On omitting certain of these coefficients we may obtain approximations, and the analysis just given shows the degree of error involved in these approximations. For instance, equation (13) shows

* *Zeitschr. für Phys.*, **7**, 356 (1921). † *M.N.*, **84**, 670, equation (18).

that the omission of C involves an error of the order of ϵ/k, equation (13) shows that the omission of D involves an error of the order of

$$\frac{d}{d\sigma}\left(\frac{\epsilon}{k}\right),$$

and so on.

7. Professor v. Zeipel's analysis starts from Schwarzschild's equation

$$\frac{dI}{d\sigma}+I=\mu T^4.$$

The solution of this equation is

$$I=\mu T^4-\frac{d}{d\sigma}(\mu T^4)+\frac{d^2}{d\sigma^2}(\mu T^4)-\frac{d^3}{d\sigma^3}(\mu T^4)+\ldots \quad (14)$$

For generality, additional terms which vanish in the star's far interior should be included on the right of this equation. These terms enable us to satisfy the boundary condition at the star's surface, but are properly omitted from discussion here.

Instead of using the complete solution, v. Zeipel confines himself to the first three terms on the right. Thus he assumes that

$$\frac{d^3}{d\sigma^3}(\mu T^4)=\frac{d^4}{d\sigma^4}(\mu T^4)=\ldots=0,$$

or that

$$\mu T^4=\alpha+\beta\sigma+\gamma\sigma^2 \quad (15)$$

where α, β, γ are constants. Since $\mu T^4=A$, we find from equations (2)–(6) as a consequence of assumption (15) that

$$\left.\begin{aligned} B&=\xi\sigma+\eta \\ C&=\theta \\ D=E=F&=\ldots=0 \end{aligned}\right\} \quad (16)$$

where ξ, η, θ are constants. There is again a complementary solution which enables us to satisfy boundary conditions, but this, as before, may be disregarded. Equation (6) gives at once

$$\frac{dB}{d\sigma}=\text{constant}$$

or, by comparison with (2),

$$\epsilon/k=\text{constant} \quad (17)$$

which agrees with the result obtained in § 4. Thus v. Zeipel's analysis is only valid for stars for which ϵ/k is constant. For other stars it introduces an error of the order of the variations in ϵ/k.

8. v. Zeipel does not follow this method, but transforms his equation for ϵ (our equation (10)), so that ϵ appears as the sum of two terms, namely,

$$\epsilon=-\frac{\mu}{3\rho}\left\{\frac{1}{\kappa\rho}\frac{dT^4}{d\Phi}\Delta\Phi+\frac{d}{d\Phi}\left(\frac{1}{\kappa\rho}\frac{dT^4}{d\Phi}\right)\left(\frac{d\Phi}{dn}\right)^2\right\}.$$

Calling these two terms p and q, and putting $q=a(\beta+u)$ where a and β are constants over a level surface but u is variable, the equation assumes the form

$$\epsilon=p+a(\beta+u) \quad . \quad . \quad . \quad . \quad . \quad (18)$$

v. Zeipel assumes that ϵ must be constant over a level surface (Φ=cons.). But p is already constant, so that $a(\beta+u)$ must be constant, which requires that $a=0$, and his theorem follows in the form $\epsilon=p$.

But the full equation is, in place of (18),

$$\epsilon=p+a(\beta+u)+o \quad . \quad . \quad . \quad . \quad (19)$$

where o represents the terms omitted by v. Zeipel. These are of the same order of magnitude as au, so that ϵ can be made constant over a level surface by making

$$au+o=f(\Phi),$$

where $f(\Phi)$ is any function of Φ. Equation (19) now becomes

$$\epsilon=p+a\beta+f(\Phi)$$

and the theorem has disappeared entirely. It should be noticed that the error in the theorem $\epsilon=p$ is not merely one of the order of the omitted terms o, but of the order of $a\beta$, which in turn is of the order of ϵ itself.

9. In conclusion, it may be useful to point out that the much-used equation of radiative equilibrium

$$H=\tfrac{4}{3}\pi B=-\frac{4\pi}{3k\rho}\frac{d}{dx}(\mu T^4) \quad . \quad . \quad . \quad . \quad (20)$$

is in error to the same extent as the equations just discussed. Our accurate equation (3) shows the true equation of transfer to be

$$H=\tfrac{4}{3}\pi B=\frac{4\pi}{3k\rho}\frac{d}{dx}(A+\tfrac{2}{5}C) \quad . \quad . \quad . \quad . \quad (21)$$

which only reduces to equation (20) when C is constant, *i.e.* when ϵ/k is constant.

If we introduce a fictitious temperature T′ defined by

$$A+\tfrac{2}{5}C=\mu T'^4 \quad . \quad . \quad . \quad . \quad . \quad (22)$$

equation (21) becomes formally the same as (20), except that the fictitious temperature T′ replaces the true temperature T.

From equations (22) and (9) the difference between the two temperatures is given by

$$\mu(T'^4-T^4)=\tfrac{2}{5}C.$$

In many problems the difference between T′ and T may safely be neglected, but it is obviously necessary to exercise discretion in the use of equation (20).

Note on the Distances and Structure of the Spiral Nebulæ.

By J. H. Jeans.

1. Results recently published by Hubble * and Shapley † seem to establish the inaccuracy of estimates I made some time ago of the distances and other quantities associated with the spiral nebulæ. Hubble estimates the distance of M. 31 (the Andromeda nebula) as 950,000 light-years, as against my estimate of 5000 light-years.‡ Even apart from this, however, the time has come when my calculations may reasonably be revised in the light of recent nebular knowledge.

The calculations in their original form appeared in my *Cosmogony and Stellar Dynamics*.§ From the theoretical formula

$$l=\tfrac{1}{2}\left(\frac{\pi}{\gamma\rho}\frac{dp}{d\rho}\right)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

for the distance apart l of the condensations in nebular arms I deduced the relation

$$C=l\omega \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where C is the molecular velocity and ω the angular velocity of rotation of the nebula. Calculating $l\omega$ for the Andromeda nebula from Pease's spectroscopic determination of its line-of-sight velocities at various points, I deduced for C the value $1{\cdot}44\times10^5$ cm. a second.

In the theoretical formulæ (1) and (2), l denotes the distance between successive condensations at the time they are formed, that is to say, when they are close to the nucleus. The numerical value for l inserted in formula (2), however, referred to observed condensations at the outer edge of the nebula. A recent investigation by Reynolds ‖ indicates that these two values of l may be widely different; the condensations move further apart from one another as they move outwards. In the only nebula he has so far studied in detail the increase of separation is represented by a factor of about 3. If we assume a similar factor for the Andromeda nebula, we must reduce the value of l previously inserted in formula (2) to one-third of its value, and so obtain

$$C=4{\cdot}8\times10^4 \text{ cm. a second} \quad . \quad . \quad . \quad . \quad (3)$$

2. Formula (1) cannot be made to give the distance of the nebula. For this one further datum is required. My original calculations made use of v. Maanen's determinations of the angular velocities of the nebulæ. Recently Eddington ¶ has drawn attention to the close correlation between the luminosities and masses of actual stars. If the same correlation is assumed to hold for the stellar condensations in a nebula, we can dispense with v. Maanen's measurements altogether and (in theory at least) determine the distance of the nebula from the

* *Pop. Astron.*, 1922 Feb. and March.
‡ *Halley Lecture*, p. 192.
‖ *M.N.*, **85**, 142.
† *Harvard Coll. Obs. Circ.*, 260.
§ Page 217.
¶ *M.N.*, **84**, 308.

observed stellar magnitudes of its condensations. The method is as follows.

3. Let a nebula be at a distance of q parsecs, and let adjacent condensations, actually at a distance l apart, appear after correction for foreshortening to be at a distance s seconds of arc apart in the sky. Then

$$l=\frac{qs}{206{,}000}\text{ parsecs}=1{\cdot}5\times10^{13}qs\text{ cm.}$$

The theoretical formula (1) gives (approximately)

$$l=\tfrac{1}{2}C\gamma^{-\frac{1}{4}}\rho^{-\frac{1}{2}},$$

so that the mass M of a condensation is given by

$$M=l^3\rho=l(l\rho^{\frac{1}{2}})^2=\frac{1}{4}\frac{lC^2}{\gamma}=5{\cdot}6\times10^{19}C^2qs,$$

or, in terms of the sun's mass (2×10^{33} gm.),

$$M=2{\cdot}8\times10^{-14}C^2qs \qquad . \quad . \quad . \quad . \quad (4)$$

Suppose that the condensations in the nebular arms appear to be of magnitude $5{\cdot}1+n$, or n magnitudes fainter than the sun would appear at a distance of 10 parsecs. Then the luminosity of a condensation, in terms of that of the sun, is given by

$$L=q^2 10^{-(2+0{\cdot}4n)} \qquad . \quad . \quad . \quad . \quad (5)$$

If Eddington's mass-luminosity correlation is used to provide a further relation between L and M, we can eliminate L and M between this relation and our two equations (4) and (5). In this way we obtain q in terms of C and s. The value of C can be obtained in the way explained in § 1, while s is estimated from direct observation, so that q is fully known.

The method, although simple in theory, may encounter in practice a difficulty which may render it almost valueless. The variable stars from which Hubble estimates his distances appear to be of absolute magnitude about -4,* and for stars of this magnitude the mass-luminosity correlation is such that L varies approximately as $M^{1{\cdot}8}$. Had the relation been that L varied as M^2, the method would have been useless, for the elimination of L and M from equations (4) and (5) would have eliminated q also. As things stand, the elimination of L and M leaves a formula which shows that q varies as C^{20}. Since the value

* This statement contains the answer to Professor Shapley's criticism of my theory of the origin of the stars (*H.C.O. Circular*, No. 257). He considers the particular case of M. 33, and states that with an assumed distance of 2000 parsecs (Van Maanen's estimate) the average condensation would be of absolute magnitude fainter than +6. If so, with an assumed distance of 310,000 parsecs (Hubble's approximate estimate) the average condensation would be of absolute magnitude fainter than -5, a result which Professor Shapley could hardly regard as subversive of my theory. The controversy does not appear to be one between Professor Shapley and myself, so much as one between the estimates of Van Maanen and Hubble as to nebular distances.

of C may quite well be in error by a factor of 2, it appears that the resulting value of q may be in error by a factor of over a million.

The analysis can more suitably be used in the reverse direction to obtain a value for C. It is hardly necessary to assume a value for q at all. With any reasonable value for q the absolute magnitude of the condensations will be in the neighbourhood of -4, and here the relation between mass and luminosity approximates fairly closely to $L=10M^2$, whence we obtain

$$10^{-1\cdot5-0\cdot2n}=2\cdot8\times10^{-14}C^2s.$$

For the Andromeda nebula we may take $s=1$ (admittedly a very rough estimate) and $n=13$, and obtain

$$C=5\cdot4\times10^4 \text{ cm. a sec.} \quad . \quad . \quad . \quad . \quad (6)$$

which agrees well with the value for C found in equation (3).

4. The two values we have obtained for C are both of them of the order of magnitude of molecular velocities in a gas. This appears to provide a satisfactory confirmation of the hypothesis, embodied in equation (1), that nebular condensations are formed by gravitational instability in a gas. The exact velocity 5×10^4, which is appropriate to a gas of molecular weight 28 at a temperature of 40° C., is perhaps rather smaller than might be expected if the nebular arms are supposed to be wholly gaseous. Let us make the more general supposition that the mass ρ in a unit volume consists of a mass $\theta\rho$ in the gaseous state together with a mass $(1-\theta)\rho$ in solid or liquid form. The pressure is now $\frac{1}{3}\theta\rho C^2$, so that equation (6) must be replaced by

$$\theta^{\frac{1}{2}}C=5\cdot4\times10^4 \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This equation would seem to permit of anything up to about 90 per cent. of the matter in the arms being in the solid or liquid state.

5. If the lenticular shapes of the central nuclei of the nebulæ are supposed to be caused by rotation, the angular velocity ω must in any case be comparable with $(\gamma\rho)^{\frac{1}{2}}$, where γ is the gravitation constant and ρ is the mean density of the whole nucleus. To a fair approximation we may suppose the nucleus to be of the critical shape of a sharp-edged lens, in which case the precise relation is

$$\omega^2=0\cdot35\times2\pi\gamma\rho,$$

giving

$$\rho=\frac{3\times10^8}{P^2} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where P is the period in seconds.

For the Andromeda nebula Pease's formula * for the line-of-sight (spectroscopic) velocity,

$$v=-0\cdot48x-316 \text{ km. a sec.},$$

* *Nat. Acad. Sci.*, **4** (1918), 21.

in conjunction with Hubble's estimate of 950,000 light-years for the distance of the nebula, fixes the period of rotation as

$$P = 5{\cdot}7 \times 10^{14} \text{ sec.},$$

or 18 million years. Equation (8) now gives

$$\rho = 9 \times 10^{-22}.$$

If the nucleus is wholly gaseous, with a mean molecular weight of 28, the mean free path is about 100 million km.; if the nucleus is partially solid or liquid, the free path is correspondingly greater.

The diameter of the nucleus is about 12 minutes of arc, or $1{\cdot}57 \times 10^{21}$ cm. at 950,000 light-years. From this and the value of ρ just given we find that the mass of the nucleus must be 5×10^{42} grammes, or about 2500 million suns, which is significantly near to the estimated total mass of our galactic system.

The equatorial cross-section of the nebula is $7{\cdot}8 \times 10^{42}$ sq. cm., so that the mass is distributed at the rate of about 1 gramme per sq. cm. of cross-section in the line of sight. This gives an optical thickness which averages 1 gramme per sq. cm. With such a value for the optical thickness, it is clear that the nebula would be almost entirely transparent if made wholly of gas, or of solids of dimensions of several centimetres or more, or of a mixture of the two; it would be opaque only in so far as it consists of particles of size less than a centimetre and greater than a wave-length.

The nucleus of the Andromeda nebula is of stellar magnitude about 6·0, and this, at 950,000 light-years, represents an emission of light equal to 400 million times that of the sun. As we have estimated the mass of the nucleus at 2500 million times that of the sun, the emission of radiation per unit mass, in terms of that of the sun, is only 0·16. This may be compared with Oepik's estimate * of 0·38 for our galaxy; it may be contrasted with the factor of 125, which is appropriate to a star of absolute magnitude −4 and mass twenty times that of the sun.

It is difficult at present to discover the proper cosmogonic interpretation of these figures. The most obvious interpretation would be that the nucleus of the Andromeda nebula consists of a mixture of stars somewhat like that which forms our own stellar system, in which case the outer regions also must be supposed to consist of fully developed stars. It is difficult to imagine that those nebulæ which exhibit a lenticular centre and filamentous arms with pronounced condensations can have a similar constitution. It seems more likely that the Andromeda nebula may be in a far later stage of development than the typical spiral; it may exemplify a state intermediate between the typical spiral and the galactic system.

* *Ap. Journ.*, **55** (1922), 409.

On the True Shape of some Spiral Nebulæ.

By H. Groot.

1. Although we know a great many spiral nebulæ, there is not complete agreement as to their true shapes. Following the investigations of von der Pahlen * on M. 33, M. 74, and M. 51, the idea that the spiral arms are equiangular spirals seems to have gained ground; but, on the other hand, it has also been stated that a general law of formation is out of the question; also in his work on cosmogony, *Origine et Formation des Mondes*, Abbé Moreux makes the astonishing remark that the *lituus* or spiral of Cotes, satisfying the equation $r^2\theta=a^2$, gives the most correct representation of the shape of the nebulæ.

There seems to be no general unanimity on this subject, and, however unsatisfactory it may appear, this state of affairs can be easily accounted for by anyone who has become familiar with photographs of nebulæ. At first sight it appears well-nigh impossible to represent these rarefied, vague contours, which are repeatedly interrupted by condensations, by a mathematical curve. On closer examination, however, this is found to be not so hopeless. We can try to find a curve representing the spiral structure as well as possible. Only, if it appears that in the choice of this curve there is too much ambiguity, *i.e.* only if the same nebula can be equally well represented by various spirals, need we admit that the problem is insoluble. But this is not the case. Von der Pahlen has shown that the arms of the three nebulæ above mentioned could be well represented by equiangular spirals only. I myself have come to the same result for about eight other nebulæ.

2. The method which I have followed in my investigations is more or less the same as that of von der Pahlen. It amounts to this. The shape of the given nebula is drawn as well as possible on tissue paper. Through the centre of the diagram straight lines are drawn at angles of 30°. The radii vectores, ρ_0, from this centre to the respective points of the curve are measured at different position-angles, θ.

Now the curve which has been drawn is the projection of the true curve on a plane perpendicular to the line of sight. The first problem, therefore, is to find the line of nodes. To this end the values found for ρ_0 and θ are plotted in a system of rectangular co-ordinates. Whenever there is a noticeable inclination, ρ_0 will not decrease gradually with θ, but show a sine-like increase and decrease with a period of 180°. Now it is easily proved that

$$\left.\begin{aligned} r&=\rho(1+\tan^2\omega\sin^2\theta)^{\frac{1}{2}} \\ \tan\phi&=\tan\theta\sec\omega \end{aligned}\right\} \quad . \quad . \quad . \quad (1)$$

where ω is the inclination, ϕ the position-angle in the plane of the spiral, and r the radius vector in that plane. Hence $\rho=r$ if $\theta=0°$, 180°, 360°, . . . ; ρ being a maximum at the same angles.

After finding these maximum values for ρ_0 in the diagram, the direction of the line of nodes is easily computed.

* *A.N.*, 4503.

We now make a hypothesis about the mathematical formula of the spiral, *e.g.* that it is an equiangular spiral :

$$\log r = A - B\phi \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From the values of ρ_0 for $\theta=0$ and 180° we can obtain A and B (when $\theta=0°$, 180°, . . ., $\rho_0=r$).

With the aid of these constants, we now calculate r for $\theta=90°$, 270°, . . . in order to find the value of ω from the first equation (1), which for these values of ω changes into

$$\cos\omega = \frac{\rho_0}{r_c} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

(the suffix o means *observed* value, *c computed* value, and *t true* value).

Now that the values for A, B, and ω are known, the values of ρ_c for $\theta=0°$, 30°, 60°, . . . can be calculated and compared with the observed ρ_0. If the spiral nebula is to be represented by the formula thus found, the differences $\rho_0-\rho_c$ must not be too large.

3. The greatest difficulty of the process lies in the drawing of the curve which has to represent the shape of the nebula. At first sight one might fear too great a range in the possibilities of drawing. But I have been able to ascertain, by drawing the same nebula several times with intervals of some weeks, without consulting the previous diagrams, that, taking everything into consideration, the differences are not such as to make the process illusory. In the diagrams drawn from the photographs in the *Publ. Lick Obs.*, **8**, which have a maximum diameter of about 3 or 4 cm. on the average, differences of 1 or 2 mm. are to be expected in the worst-defined parts, but I do not believe that errors of more than 3 mm. are unavoidable.

Unlike von der Pahlen, who always gives tenths of mm., I have always given halves of mm. ; greater accuracy cannot be obtained in any case.

Besides, by errors in the drawing the values found for ρ_0 may also be influenced by the three following systematic errors :—

(*a*) *Error of Inclination.*—The inclination, ω, is found from the value of $\frac{\rho_0}{r_c}$ for $\theta=90°$, 270°, . . . In general these values differ amongst themselves, therefore it will be necessary to take a certain average value ω in the calculation. Of course this value may be too great or too small. If ω is too great, then $\rho_c<\rho_0$, except for $\theta=0°$, 180°, . . ., when the error is zero.

In general the amount of this error is not great ; it increases with ω.

(*b*) *Error in the Line of Nodes.*—There is a possibility, especially in the case of small inclinations, that there is a difference of $\alpha°$ between the true line of nodes and the one assumed in the diagram (so that $\alpha°$ should be added to the observed θ). In general this error will not be very important, as α is not greater than 15° except in the case of small inclinations, and the influence only becomes important for large values of ω.

(*c*) *Error of the Origin.*—In the case of nebulæ with a vague nucleus (*e.g.* M. 81) it may happen that not the chosen centre, O, but another point, O′, is the real centre of the spiral. In this case, too, the measured

ρ_0 will differ from the true value. Presumably this error will not be more than 0·5 mm. in any of the cases considered.

In order to demonstrate the influence of the last two errors mentioned,* the projection of the spiral $\log r = 1{\cdot}0000 - [9{\cdot}0000]\phi$ was drawn, assuming $\omega = 60°$, and the values for ρ_0 measured, allowing for (*a*) a central error of 0·3 mm., and afterwards for (*b*) an error of 15° in the line of nodes. For these values of ρ_0 new equiangular spirals were computed:

$$(a)\ \log r = 0{\cdot}9868 - [8{\cdot}9720]\phi\ ;\quad \bar{\omega} = 60°\ ;$$
$$(b)\ \log r = 0{\cdot}8921 - [8{\cdot}8080]\phi\ ;\quad \bar{\omega} = 60°.$$

The differences $\rho_0 - \rho_c$ remain less than 0·6 mm. in either case.

These errors have, therefore, slightly modified the constants of the spiral, but the mean inclination as well as the general character have been preserved, no other spirals being possible. Thus we need not worry too much about these errors. On the other hand, it appears that through combination of these errors differences of 1 mm. between ρ_0 and ρ_c are possible.

If we remember that in addition to this there may also be errors in inclination and in drawing, it is my opinion that we may estimate the mean error in ρ_0 at 2 mm. without having to doubt the general similarity between the true curve and the calculated one.

4. Nine nebulæ were investigated in the manner described above, viz. M. 51, H.V. 43, H.I. 199, N.G.C. 4321,* H.I. 84, M. 100, M. 88, M. 65, and M. 81.

The equiangular spiral, the Archimedean spiral, and the *lituus* were all considered. The nine spirals (17 spiral arms) could all be properly represented by equiangular spirals. Only M. 65, I. and II., M. 81, I. and II., and M. 100, I. can also be interpreted as Archimedean spirals or *lituus*. Very likely this is caused by the fact that these nebulæ could only be investigated within a relatively limited range. As all other nebulæ gave equiangular spirals, I have felt justified in using the same form in these doubtful cases.

In Tables I. and II., which give a summary of the results, I have given the results for the equiangular spirals only. In Table I. I have inserted all the values obtained for ρ_0 with the corresponding values of θ and the differences $\Delta = \rho_c - \rho_0$. Table II. needs little explanation. In the second column the numbers between brackets represent the logarithms of the coefficients of ϕ, measured in radians, increased by 10. In the column under β are inserted the angles between the radius vector and the tangent. The next two columns show the maximum and minimum for ρ_0 obtained in the different nebulæ, in mm. The column under n gives the number of points by which the spiral is defined. The next two columns indicate how many times $\Delta = \rho_c - \rho_0$ exceeded 1 mm. and 3 mm. respectively, and the following column under ϵ denotes the mean error of one observation.

The photographs I have used have been taken from Jeans' *Problems of Cosmogony* (1919), indicated by J, from the *Publ. Lick Obs.*, **8** (L), and from the *Adolfo Stahl Lectures* (1919), marked A.

* After this paper was in print Mr. Reynolds pointed out that M. 100 and N.G.C. 4321 are the same object. A comparison of the measures indicates the accuracy with which the form of a spiral nebula can be measured.—*The Secretaries.*

TABLE I.

θ.	M. 51. I. ρ_0.	M. 51. I. Δ.	M. 51. II. ρ_0.	M. 51. II. Δ.	H.V. 43. I. ρ_0.	H.V. 43. I. Δ.	H.V. 43. II. ρ_0.	H.V. 43. II. Δ.	H.I. 199. I. ρ_0.	H.I. 199. I. Δ.	H.I. 199. II. ρ_0.	H.I. 199. II. Δ.	N.G.C. 4321. I. ρ_0.	N.G.C. 4321. I. Δ.	N.G.C. 4321. II. ρ_0.	N.G.C. 4321. II. Δ.	H.I. 84. I. ρ_0.	H.I. 84. I. Δ.	H.I. 84. II. ρ_0.	H.I. 84. II. Δ.
−180	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	22·5	+0·6
−150	..	..		..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	16·0	−0·7
−120	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	14·0	−1·9
− 90	..	..	..	..	..	..	..	..	..	..	9·0	+0·6	..	..	21·0	−1·2	..	..	11·5	−0·7
− 60	..	..	..	..	..	..	..	..	..	..	9·5	+1·0	19·5	−1·1	18·5	−0·4	..	..	11·5	0
− 30	..	..	..	..	..	..	..	..	..	..	13·5	+2·1	17·5	0	16·5	+0·4	..	..	14·0	−0·7
0	48·0	0	37·0	0	45·0	0	43·0	0	20·0	0	24·0	0	16·0	0	15·0	0	23·0	0	14·5	0
+ 30	32·0	+7·1	29·0	+2·0	18·0	+2·2	16·0	+4·8	9·5	−0·5	14·0	−5·8	13·5	+0·5	13·5	−0·6	15·5	−0·1	11·0	+1·1
+ 60	25·5	+4·7	23·0	+1·5	11·5	+1·1	10·5	+2·8	6·0	−0·6	7·0	−2·2	12·0	−0·1	12·0	−1·4	11·0	−0·3	8·0	+1·6
+ 90	24·0	+1·5	21·0	+0·2	10·0	+0·4	9·0	+2·2	4·5	0	5·5	−1·1	10·5	0	9·0	+0·2	9·5	−0·2	7·0	+1·6
+120	26·0	−1·9	21·0	−0·4	11·0	0	10·0	+2·0	4·5	+0·5	6·0	−1·7	9·5	+0·3	8·0	+0·5	10·0	−0·1	7·0	+2·1
+150	27·0	−2·0	22·0	0	15·5	−1·4	15·5	+0·2	6·0	+1·1	7·5	−1·0	8·5	+0·9	7·0	+0·9	12·5	+0·2	8·0	+2·6
+180	24·5	0	22·0	−0·1	18·0	0	21·5	0	10·5	0	10·0	0	8·5	0	7·0	0	15·0	0	11·5	0
+210	19·5	+0·5	29·0	−0·6	8·0	+0·2	10·5	0	5·0	−0·3	4·5	−0·9	8·5	−1·0	6·5	−0·5	12·5	−2·3	9·0	+0·6
+240	15·0	+0·4	15·5	−1·0	5·0	0	6·5	+0·1	3·0	−0·1	2·0	0	7·0	−1·1	5·0	−0·5	9·5	−2·6	6·5	+1·0
+270	13·5	−0·5	13·0	−0·4	4·5	−0·3	5·5	+0·1	2·5	−0·1	1·5	+0·2	5·0	+0·6	4·5	−0·1	8·0	−1·9	5·5	+1·3
+300	12·5	−0·2	12·0	+0·3	5·5	−1·1	7·0	−1·0	3·0	−0·4	2·0	−0·2	4·5	+0·3	4·5	−0·6	8·0	−1·5	..	..
+330	12·0	+0·7	11·5	+1·5	7·5	−1·9	10·5	−2·6	4·0	−0·3	2·0	+0·7	4·5	+0·4	4·0	−0·4	8·5	−0·2	..	..
+360	12·0	−0·5	11·5	+1·5	7·5	−0·3	9·0	+1·7	4·5	+1·0	3·5	+0·6	4·0	+0·5	3·5	−0·2	9·5	+0·3	..	..
+390	10·0	+0·2	9·5	+0·5	..	..	..	..	..	..	..	..	3·5	+0·5	3·5	−0·7	6·5	+1·0	..	..
+420	8·0	−0·1	8·5	+0·1	..	..	..	..	..	..	..	..	3·5	−0·1	3·0	−0·7	4·0	+0·6	..	..
+450	6·5	+0·1	7·0	+0·5	..	..	..	..	..	..	..	..	3·0	0	2·0	0	3·0	+1·5	..	..
+480	6·0	+0·3	6·0	+1·3	..	..	..	..	..	..	..	..	2·5	+0·1	2·0	−0·2	..	..	..	..
+510	5·5	+1·0	6·5	+0·9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+540	5·5	+0·9	7·0	+0·8	..	..	..	..	..	..	..	..	..	..	..	..	4·0	+0·2	..	..
+570	5·0	+0·2	5·0	+1·8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

TABLE I.—*continued.*

θ.	M. 100. I. ρ_0.	M. 100. I. Δ.	M. 100. II. ρ_0.	M. 100. II. Δ.	M. 65. I. ρ_0.	M. 65. I. Δ.	M. 65. II. ρ_0.	M. 65. II. Δ.	M. 88. I. ρ_0.	M. 88. I. Δ.	M. 81. I. ρ_0.	M. 81. I. Δ.	M. 81. I. Δ'.	M. 81. II. ρ_0.	M. 81. II. Δ.	M. 81. II. Δ'.
−180	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
−150	..	..	..	..	..	..	..	..	9·5	+1·7	..	..	..	..	..	..
−120	..	..	..	..	..	..	..	..	6·0	+0·5	..	..	..	..	..	..
− 90	..	..	17·0	+1·7	..	..	..	..	5·5	−0·1	37·0	−8·7	+0·2	35·0	−9·5	−0·2
− 60	15·0	−1·0	16·0	+1·2	..	..	..	..	5·5	+0·4	36·0	−5·2	+1·0	34·0	−6·0	+1·0
− 30	14·5	−0·9	15·0	+1·2	..	..	..	..	8·5	+0·2	43·0	−3·2	+0·4	39·5	−3·0	+1·1
0	12·5	0	14·5	0	18·0	0	20·0	0	13·0	0	50·5	0	0	45·0	0	0
+ 30	11·0	−0·5	12·5	−0·5	7·5	−0·4	9·0	+0·4	6·5	−2·1	33·5	+2·3	+2·0	33·5	−2·0	+1·8
+ 60	9·0	−0·2	10·0	−0·3	5·0	−0·7	5·5	+0·2	3·5	−1·0	22·5	+3·0	+1·5	24·5	−2·0	+1·0
+ 90	8·0	−1·1	8·5	0	4·0	−0·3	4·5	+0·3	3·5	−0·9	20·0	+2·5	−0·3	21·0	−1·0	+0·6
+120	7·5	−0·2	8·0	−0·3	4·5	−0·4	5·0	+0·4	3·0	−0·7	21·0	+3·4	0	22·5	−1·0	−0·4
+150	7·0	0	7·0	+0·1	7·0	−0·8	8·0	−0·2	3·5	−0·1	27·5	+4·7	+1·0	29·5	−1·0	−1·1
+180	6·5	0	6·5	0	11·0	0	12·0	0	5·0	0	40·0	0	0	35·0	0	0
+210	5·5	0	5·5	−0·1	4·5	0	5·0	+0·6	2·5	−0·7	27·0	+1·3	−2·9	23·0	+1·0	+0·7
+240	4·0	+0·6	5·0	−0·6	2·5	+0·1	3·5	−0·1	1·5	−0·6	..	..	..	..	..	..
+270	..	..	4·0	−0·2	2·0	+0·2	3·0	−0·1	1·0	−0·2	14·0	+3·8	0	14·0	+3·0	−0·5
+300	..	..	3·0	+0·5	2·5	0	3·0	+0·2	..	..	..	..	..	..	..	..
+330	..	..	2·5	+0·8	4·5	−0·7	4·7	+0·2	..	..	..	..	..	..	..	..
+360	..	..	2·5	+0·4	6·5	+0·2	7·5	−0·3	..	..	..	..	..	..	..	..
+390	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+420	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+450	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+480	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+510	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+540	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
+570	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

TABLE II.

Nebula.		Formula of the Spiral.	ω.	β.	$\rho_{max.}$	$\rho_{min.}$	n.	$\Delta>1$.	$\Delta>3$.	ϵ.	Taken from
M. 51	I.	$\log r=1\cdot6812-[8\cdot9683]\phi$	42°	78	48·0	5·0	20	5	2	2·1	J
	II.	$\log r=1\cdot5682-[8\cdot8566]\phi$	44	81	37·0	5·0	20	4	0	1·0	J
H.V. 43	I.	$\log r=1\cdot6532-[9\cdot1027]\phi$	69	74	45·0	4·2	13	5	0	1:0	L
	II.	$\log r=1\cdot6335-[8\cdot9816]\phi$	69	78	43·0	5·5	13	7	1	2·0	L
H.I. 199	I.	$\log r=1\cdot3010-[8\cdot9498]\phi$	72	78	20·0	2·5	13	1	0	0·5	L
	II.	$\log r=1\cdot3802-[9\cdot0829]\phi$	75	74	24·0	1·5	16	5	1	1·9	L
N.G.C. 4321	I.	$\log r=1\cdot2041-[8\cdot9406]\phi$	27	79	19·5	2·5	19	2	0	0·6	A
	II.	$\log r=1\cdot1761-[9\cdot0227]\phi$	23	76	19·8	1·8	20	2	0	0·6	A
H.I. 84	I.	$\log r=1\cdot3617-[8\cdot7715]\phi$	60	82	23·8	3·0	17	5	0	1·1	L
	II.	$\log r=1\cdot1614-[8\cdot5069]\phi$	53	86	22·5	5·5	16	7	0	1·3	L
M. 100	I.	$\log r=1\cdot0965-[8\cdot9562]\phi$	30	78	15·0	4·0	11	1	0	0·6	L
	II.	$\log r=1\cdot1614-[9\cdot0451]\phi$	30	76	17·0	2·5	17	3	0	0·8	L
M. 65	I.	$\log r=1\cdot2553-[8\cdot8331]\phi$	75	81	18·0	2·0	13	0	0	0·4	L
	II.	$\log r=1\cdot3010-[8\cdot8489]\phi$	72	81	20·0	3·0	13	0	0	0·3	L
M. 88	I.	$\log r=1\cdot1139-[9\cdot1209]\phi$	74	73	13·0	1·0	15	2	0	0·9	L
M. 81	I.	$\log r=1\cdot7033-[8\cdot5080]\phi$	69±	86	50·5	14·0	12	3	0	1·2	J
	II.	$\log r=1\cdot6532-[8\cdot5407]\phi$	60±	86	45·0	14·0	12	3	0	1·3	J

Remarks.

H.V. 43	I.	Structure from 300°–360° very hazy.
	II.	Structure from 300°–360° rather hazy.
H.I. 199	II.	Structure hazy. Value of ρ_0 uncertain for $\theta=0°$, 30°, and 360°.
N.G.C. 4321	II.	Structure uncertain in the neighbourhood of $\theta=270°$.
H.I. 84	I.	Structure difficult to follow from $\theta=210°$–300°.
	II.	Not very reliable. ρ_0 for $\theta=-180°$ is extrapolated.
M. 100	II.	Structure hazy. $\omega=30°$ is taken as mean of 38°, 27°, 23°.
M. 88	I.	Structure rather hazy. Structure of arm II. could not be traced.
M. 81	I.	ω is supposed to be variable.
	II.	ω is supposed to be variable.

When we consider the results it appears from the list of mean errors that the formulæ obtained deserve more credence than we might have expected at first. In not a single case does the mean error exceed 2·1 mm., while in ten cases $\epsilon<1\cdot0$ mm.

It is perhaps possible that the constants obtained for the spirals may be slightly different from the true constants, but the ω and β obtained will not be affected by this at all appreciably, and also the conclusion that the equiangular spiral does fairly well represent the true shape of the nebulæ investigated, is not invalidated.

The small range of the value of β (from 73° to 86°) for the different nebulæ is very remarkable. It should also be pointed out that the values of β for the two arms of the same nebula, which were determined entirely independently, are in all cases very nearly the same (greatest difference =4°).

One more point deserves attention. In what has been said above

we have always supposed that the nebulæ are plane—an opinion principally founded on the photographs of the so-called "spindle" nebulæ such as N.G.C. 5746 or N.G.C. 4565, which are interpreted as spiral nebulæ seen edgewise. Is this hypothesis confirmed by our calculation? Generally speaking, it is. The differences in ω between the two arms of a nebula are not larger than 3° or 4°—with the exception of H.I. 84, where the difference amounts to 7°—an amount which probably is still smaller than the accidental errors. Besides, every arm by itself can be well represented by a plane curve. This is all the more remarkable as it does not seem to be the case with M. 81. In Table I. we find two columns of differences for M. 81. In the first column are inserted the values obtained with $\omega=60°$, in the second with $\omega=49°$ for $\theta=-90°$, with a gradual increase to $\omega=67°$ for $\theta=270°$. These columns are self-evident. I believe that they can only be interpreted as an indication that the spiral arms of M. 81 are skew curves.

Bussum:
1925 *February.*

On the Distribution of Hydrogen in a Star. By S. Rosseland.

(*Communicated by Professor A. S. Eddington, F.R.S.*)

In many astrophysical phenomena hydrogen seems to play an outstanding part. This is perhaps most conspicuous for the phenomenon of bright-line emission from gaseous nebulæ and early-type stars, where hydrogen (and helium) lines predominate in the spectra almost exclusively. For stars of later types there is a conspicuous difference between the intensities of hydrogen lines in giants and dwarfs, a difference which seems to indicate that hydrogen is much more abundant in the atmospheres of the former than of the latter.* Finally, the most interesting instance is afforded by the novæ and by the long-period variables, for which the appearance of bright hydrogen lines with mysterious peculiarities as regards intensity distribution and radial velocity is as characteristic as the light variation itself.

In this state of things it seems desirable that facts of a theoretical nature about hydrogen should also be pointed out in order that we may discover whether there is something inherently abnormal about

* Compton and Russell suggested that this might be a spurious effect, in some way due to metastability of the 2_1 orbit of the hydrogen atom. It does not seem probable, however, that this metastability would much affect the distribution over the different possible quantum orbits under those conditions. Thus in the most simple case, when three orbits only are taken into account and when the effect of collisions is neglected, it is easily found, on using the well-known Einstein relations between the transition coefficients, that the maximum concentration corresponds to temperature equilibrium at the effective temperature of the star, regardless of the distance from the star at which the gaseous atoms in question are situated. This metastability does not therefore appear to explain the effect in question. On the other hand, effects of this sort may be of essential importance for the case of gaseous nebulæ where the radiating matter is illuminated by distant stars and is probably itself at very low temperature (cf. *Nature*, **114**, 859).

hydrogen in the stars, or whether the phenomena thus observed are but logical consequences of the ordinary properties of hydrogen as observed in terrestrial laboratories. It may therefore be of interest to point out that the fact that the ratio of electric charge to mass for the hydrogen nucleus is twice as large as for any other element has the necessary consequence that the spatial distribution of hydrogen in a star may differ radically from that of any other element. In fact, while other elements may be expected to show maximum concentration at the stellar centre, hydrogen may show exactly the reverse, and be most strongly concentrated in surface regions of the star. To what extent this circumstance may be made answerable for the behaviour of hydrogen in the stars it may be difficult to say definitely, but it seems to be a characteristic theoretical feature which must be borne in mind when theories on this or allied subjects are formed.

Hydrostatic Equilibrium of a Gaseous Star.—When considering the law of distribution of different elements throughout a gaseous star in hydrostatic equilibrium it is necessary to distinguish sharply between two different cases. In the case of *true* equilibrium mass motion of any kind is absent, and it is then possible to distinguish between the different constituent gases, which in a certain sense will be distributed independently of one another. In the second case the matter will only be in a kind of pseudo-equilibrium, as there may be convection currents present which tend to enforce a continual mixing up of the constituent gases. Such is, for instance, the case in the terrestrial troposphere, and the same is undoubtedly the case in the solar atmosphere, and probably in the atmospheres of most stars. The principal problem, however, is what kind of equilibrium is prevalent in the main *interior* of a star. This question leads to the consideration of the *stability* of the state which corresponds to true hydrostatic equilibrium in the sense considered above. If this latter is stable there is no possibility for convection currents to originate; if not, such currents will inevitably be set up, and although they may be ineffective in changing the law of distribution of temperature and density which corresponds to true equilibrium, they may nevertheless be effective in mixing up completely the different constituents of the star. The problem of the hydrostatic equilibrium of a star involves an unknown but very important datum: the distribution of the sources of stellar energy. This latter directly determines the distribution of temperature in the stellar interior; if the energy is generated near the stellar surface the inner core will be isothermal —as it is only heated from without —and the equilibrium will necessarily be of the stable kind; as the energy is supposed to be generated closer and closer to the centre a larger and larger temperature gradient will be set up, and if the adiabatic gradient is attained instability will of course set in. In Eddington's theory the rate of generation of energy per unit volume is assumed to be very nearly proportional to mass density, and the gradient of temperature thus set up is probably sub-adiabatic and the equilibrium of the stable kind. We may at least contemplate the possibility that the stellar core is in stable equilibrium, and we shall now follow up some consequences of this hypothesis.

The equilibrium is brought about by the simultaneous action of gas pressure, radiation pressure, gravitational and electric forces—the latter being due to a presumed net electric volume charge of the star, the importance of which for problems of this kind was pointed out on an earlier occasion.* The light pressure will presumably act primarily on the free electrons, as these latter, by the expulsion from the atoms in the absorption processes, carry off practically all the momentum of the absorbed radiation. There will, however, also be a certain light pressure on the atoms due to absorption of radiation in a line spectrum, but this will be neglected here, and the main result is on the whole largely independent of the assumptions regarding the partition of radiation pressure between the constituent particles. We consider the star in question to contain r different kinds of elements, and denote the mass and mean electric charge on an atom of the i^{th} kind by m_i, $Z_i\epsilon$, by n_i the number of such atoms per unit volume, and by p_i the partial pressure of this gas. Denoting further the gravitational and electric potentials by ϕ and ψ, the mass, electric charge, and light pressure per electron by m_e, $-\epsilon$, and f, and absolute temperature by T, we may write the equations of equilibrium in the form

$$\left.\begin{aligned} &\frac{dp_i}{dr}+n_i m_i\frac{d\phi}{dr}+n_i Z_i\epsilon\frac{d\psi}{dr}=0\,;\quad p_i=n_i kT,\ i=1\,.\,2\,.\,.\,.\,r\\ &\frac{dp_e}{dr}+n_e m_e\frac{d\phi}{dr}-n\epsilon\frac{d\psi}{dr}+f=0 \end{aligned}\right\} \quad . \quad (1)$$

where k is Boltzmann's constant. Distinguishing values at the centre of the star by zero index, the above equations may be written in the form

$$n_i=n_i{}^0\frac{T_0}{T}e^{-\int_0^r\left(m_i\frac{d\phi}{dr}+Z_i\epsilon\frac{d\psi}{dr}\right)\frac{dr}{kT}},\ \text{etc.}\,. \quad . \quad . \quad (2)$$

To this system must be added two equations of Poisson for the potentials

$$\nabla^2\psi=-4\pi\epsilon\left\{\sum_{i=1}^{r}n_iZ_i-n_e\right\} \quad . \quad . \quad . \quad . \quad (3)$$

$$\nabla^2\phi=4\pi G\left\{\sum_{i=1}^{r}n_im_i-n_em_e\right\} \quad . \quad . \quad . \quad . \quad (4)$$

A certain aspect of the solution of the above equations may be shown by simple considerations without regard to the exact law of temperature distribution. For this purpose it is sufficient to realise how enormously larger is the electrical than the gravitational force between two ions, which for the case of two hydrogen nuclei stand in a ratio

$$\frac{\epsilon^2}{H^2G}=1{\cdot}3\,.\,10^{36},$$

where H is the mass of a hydrogen atom.

* *M.N.*, **84**, 722.

Thus 1·5 *milligrams* of free hydrogen nuclei symmetrically distributed throughout the sun will be sufficient to keep a free hydrogen nucleus in suspension in the solar atmosphere. The material of any star in a state of equilibrium must therefore, to an exceedingly high degree of approximation, be electrically neutral in the sense that the number of free positive charges in unit volume is very nearly equal to the number of negative ones. This circumstance suggests a method of arriving at a solution by successive approximations. Writing the electrical density in the form

$$n\epsilon=\epsilon\Sigma n_i Z_i-\epsilon n_e \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and introducing expression (2), we obtain

$$e^{-\int_o^r\left(m^e\frac{d\phi}{dr}-\epsilon\frac{d\psi}{dr}+f\right)\frac{dr}{kT}}\frac{\Sigma n_i{}^o Z_i e^{-\int_o^r\left(m_i\frac{d\phi}{dr}+Z_i\epsilon\frac{d\psi}{dr}\right)\frac{dr}{kT}}-nT/T_o}{\Sigma n_i{}^o Z_i{}^o-n^o} \quad . \quad (6)$$

But we have seen that the numbers n and n_o must be exceedingly small compared to the leading terms on the right-hand side, and the left-hand exponential expression is therefore but an arithmetic mean value of the exponentials on the right-hand side so long as Z_i does not differ appreciably from $Z_i{}^o$. As the mass of an electron is negligibly small compared with the mass of any positive ion, this equation is only possible provided $\frac{d\psi}{dr}$ has the opposite sign to $\frac{d\phi}{dr}$, that is when the star carries a nett positive charge. For the case in which the star consists of one element only we obtain from (6) to a first approximation

$$\frac{d\psi}{dr}=-\frac{m_i-m_e}{\epsilon(Z_i+1)}\cdot\frac{d\phi}{dr}+\frac{f}{\epsilon(Z_i+1)} \quad . \quad . \quad . \quad (7)$$

Neglecting light pressure in this expression we get as the lower limit for ψ:

$$\psi=-\frac{m_i-m_e}{\epsilon(Z_i+1)}\cdot\phi \quad . \quad . \quad . \quad . \quad (8)$$

Assuming, as in Eddington's theory, that the force due to light pressure is proportional to gravitational force,

$$f=-(1-\beta)\frac{m_i}{Z_i}\frac{d\phi}{dr} \quad . \quad . \quad . \quad . \quad (9)$$

where β is a constant less than unity, and we find

$$\psi=-\frac{m_i(1+(1-\beta)Z_i{}^{-1})-m_e}{\epsilon(Z_i+1)}\phi \quad . \quad . \quad . \quad (10)$$

This solution corresponds by (3) to an electric volume density of

$$n\epsilon=\rho G\frac{m_i(1+(1-\beta)Z_i{}^{-1})-m_e}{\epsilon(Z_i+1)} \quad . \quad . \quad . \quad (11)$$

where ρ is the mass density and G the gravitational constant. Although we have arrived at this result by continued approximations, it is easily verified that by adjusting the ratio of $n_e^o n_i^o$ at the centre it will fit the equations exactly. The ratio required is determined by the equation

$$\frac{\epsilon}{G}\frac{n_i^o Z_i^o - n_e^o}{n_i^o m_i - n_e^o m_\epsilon} = \frac{m_i(1+(1-\beta)Z_i^{-1}) - m_e}{\epsilon(Z_i+1)} \quad . \quad . \quad . \quad (12)$$

On considering the case of a star that is built up of a number of different elements, we may write

$$\psi = -\alpha\phi,$$

where α is now variable with the distance from the centre of the star. Considering again expression (6), it will be seen that α must satisfy the inequality

$$\frac{M}{\epsilon(Z+1)} > \alpha > \frac{m}{\epsilon(z+1)} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where M, Z, m, z correspond to the heaviest and the lightest atoms present to any measurable degree in the star. We are here chiefly interested in the lower limit for α, and shall distinguish between two principal cases. In the first case the stellar core will be assumed to contain hydrogen principally. The lower limit of α will then be determined by hydrogen, and be given by

$$\alpha = \frac{H}{2\epsilon} \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

the total potential energy of a hydrogen nucleus relative to the position at the stellar centre being

$$U_H = \frac{H}{2}\phi \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where the gravitational potential is assumed to be zero at the centre. The equilibrium is then as it would be if electrical forces were absent and the nuclear mass reduced by a factor one-half. In the second case we assume the stellar core to contain only a moderate amount of hydrogen. The minimum value of α will then be given by

$$\alpha = \frac{2Z'}{Z'+1}\frac{H}{\epsilon} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where Z' is the atomic number of the lightest element present to any perceptible degree. For helium ($Z'=2$) we should obtain $\alpha = \frac{4}{3}\frac{H}{\epsilon}$, and for heavier elements the value would rapidly converge towards the limit $\alpha = \frac{2H}{\epsilon}$. A hydrogen nucleus situated in this field of force would have a potential energy relative to the centre of

$$U_H = -\left(1 - \frac{2}{Z'+1}\right)H\phi \quad . \quad . \quad . \quad . \quad (17)$$

which is seen to be *negative*, meaning that under these conditions hydrogen will be repelled from the core. Theoretically a state is possible in which hydrogen is nearly evenly distributed in the core, but it is unstable, as a small expansion will tend to increase the electric charge, and the attraction might easily change to repulsion, leading to a continued and accelerated outflow of hydrogen from the region in question. The final stable state will, of course, depend very much upon the conditions to which the boundary of the star is subjected, and which we so far have tacitly ignored. This was done because it can be shown that widely differing conditions at the boundary can influence the state in the main interior of the star only a very little. Consider, for instance, the simple example when the star consists entirely of hydrogen. Equation (3) may for this case be written in the form (on neglecting light pressure)

$$\nabla^2\psi=-\frac{H}{2\epsilon}\nabla^2\phi\left\{1-\sigma-\frac{2\epsilon^2}{H^2G}\left(e^{2\epsilon\int_0^r\left(\frac{d\psi}{dr}+\frac{\Pi}{2\epsilon}\frac{d\phi}{dr}\right)\frac{dr}{kT}}-1\right)\right\}. \qquad (18)$$

where we have written

$$1-(1-\sigma)\frac{H^2G}{2\epsilon^2}$$

for the ratio of the number of negative to positive particles at the stellar centre. For $\sigma=0$ the solution of (18) is $\psi=-\frac{H}{2\epsilon}\phi$, as considered before, and the star will have an electric potential $\psi_s=-\frac{H}{2\epsilon}\phi_s$ at the surface, where ϕ_s is the surface value of the gravitational potential. Otherwise ψ_s will be a function of σ of such a kind that very small changes in σ will correspond to very large changes in ψ_s. This follows from the fact that the additional term in (18) is multiplied by the enormously large factor $\frac{2\epsilon^2}{H^2G}$, which will magnify any deviation from equality between ψ and $-\frac{H}{2\epsilon}\phi$ 10^{36} times, and it is evident that this result must be independent of the special assumption that the star was constructed of hydrogen.

It seems that we are led to the conclusion that if the core of a star is in true hydrostatic (non-convective) equilibrium, and if it does not contain an enormous proportion of hydrogen, then hydrogen must be repelled from the core and strongly concentrated in the surrounding convective layer and at the surface.

Mount Wilson Observatory, Pasadena:
January 31.

Note on Cumulative Ionisation in Stellar Atmospheres.

By K. T. Compton.

In a recent paper on this subject, Mr. Martin C. Johnson (*M.N.R.A.S.*, **84**, 516, 1924) has discussed the applicability to astrophysics of certain equations which I derived to describe the conditions in a particular type of discharge tube (*Phys. Rev.*, **20**, 283, 1922). Although I quite agree with him regarding their failure to apply to astrophysics, I beg permission to point out that it is unjustifiable therefrom to draw conclusions regarding the rôle of cumulative ionisation in the solar atmosphere.

1. The chief discrepancy to which Mr. Johnson refers is between Fowler's (*Phil. Mag.*, **47**, 257, 1924) conclusion that the ratio of impact to photoelectric ionisation is proportional to gas-pressure and my equation giving $P_i/P_r = 1/p^2$, where the P's are the fractions of atoms in excited states as the direct result of electron impact and absorbed radiation respectively. These conclusions are based on totally different assumptions, Fowler assuming equilibrium, whereas I dealt with a case as far removed from equilibrium as could well be imagined. The logical way to understand this discrepancy is not to consider a departure from solar equilibrium, as Mr. Johnson does, but to examine the conditions which my equation was intended to describe. These were, briefly, a continuous small rate of production of excited atoms by electron impact in a restricted region of gas, emission of this radiation and successive absorption and re-emission by neighbouring atoms until the radiation reaches the walls of the containing vessel and a mean life, or duration, of an excited state. It was assumed that the energy of this resonance radiation is not changed in wave-length, but exists either as radiant energy or as potential energy of the excited states until its escape at the walls of the apparatus. Under these conditions there is a *gradient* of radiation density, or of concentration of excited atoms, from the region of their original production to the walls of the apparatus.

Now, the factor p^2 above rises solely because of this concentration gradient, which has no analogue in the solar case. In fact, it is easy to see that the assumption of conservation of energy of resonance radiation leads to the conclusion that P_r/P_i approaches infinity as the gradient becomes less, *i.e.* as equilibrium conditions are approached. This is because every "quantum" of resonance energy, originally produced by an electron impact, would remain for ever imprisoned in the gas yielding an indefinitely large number of radiative excitations.

This impossible conclusion is due to the neglect of processes which alter the frequency of the resonance radiation and result ultimately in the condition of equilibrium. These processes, which are of minor importance in the case of extreme departure from equilibrium for which I derived the equations, become of importance equal to the primary processes of excitation in determining equilibrium conditions.

2. The statement that I have used the theory to provide a relation of collision excitation to radiative excitation in a stellar atmosphere is, I fear, misleading. I have realised too well the limitations of the equations mentioned above to attempt to apply them to stellar condi-

tions. The application which I suggested, and which Russell has more fully developed (*Astrophys. Journ.*, **55**, 354, 1922), is based solely on the general idea that the presence of the high-temperature radiation from the photosphere increases the number of atoms in excited states, at the expense of the number in the normal state, to an extent depending on the amount of radiant energy available for absorption. Of course there are the compensating factors noted above which tend to re-establish equilibrium. *But the existence of the Fraunhofer spectrum is sufficient evidence that this equilibrium is not completely established,* hence the flux of radiation must act in the manner suggested. Whether this effect is sufficient to account for the phenomena under discussion is open to question.

3. Finally, Mr. Johnson has objected to my discussion of potential drop as affecting the relative importance of radiative and impact excitation, and has pointed out that the effective meanings of potential drop and temperature are identical, both leading to electron velocities. In this, however, he fails to grasp the significance of my discussion, which referred to high-speed electrons moving among low-temperature atoms in an electric field, so that the velocity of impact (and therefore the nature of effect produced) depends on the velocity gained during a free path, and hence on the pressure and field strength. In the temperature case, on the other hand, the velocities depend on temperature alone, and not on pressure. Furthermore, my discussion was based on electron velocities up to values much in excess of the ionising velocities, whereas reversing layer temperatures give relatively few electrons of such velocities.

I quite agree with Mr. Johnson's analysis of conditions in general, but fear that his discussion of these conditions in relation to my theory of cumulative ionisation may lead to an unjustifiable opinion that cumulative ionisation, due to the flux of high-temperature radiation from within, is of negligible importance in explaining departures from the equilibrium-dissociation theory of the solar spectrum.

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Determination of the Solar Rotation from Long-lived Spots.

(*Communicated by the Astronomer Royal.*)

1. Measures of Sunspots have been made at Greenwich for four complete solar cycles. In the *Daily Results*, published in the annual volumes, the positions of separate components of a group of spots have been given, but in the *Ledgers* only the positions of the centres of the groups were collected. The data of these ledgers for the years 1879–1901, *i.e.* for two solar cycles, were used by Mr. and Mrs. Maunder to determine the solar rotation (*M.N.R.A.S.*, **65**, 813). Since this publication two solar cycles have elapsed, and a further discussion with the addition of the new material seems desirable.

Instead of using spot-groups, *i.e.* the mean of a number of allied spots, long-lived single spots have been chosen. By long-lived is meant spots which have lasted for twenty-five days or longer. These spots are usually of " regular " and roughly circular form. In a very large percentage of cases such spots are the leaders of groups. Their regular shape and long duration make them very suitable for determination of the Sun's rotation.

2. The measures of spots of this kind have been extracted from the *Daily Results* since 1878 and collected in the form in which they have been published since 1916. The method of reduction is very simple. The mean position of the spot for the days when visible at its first appearance (excluding days when the spot is more than 75° from the central meridian) is compared with the corresponding mean

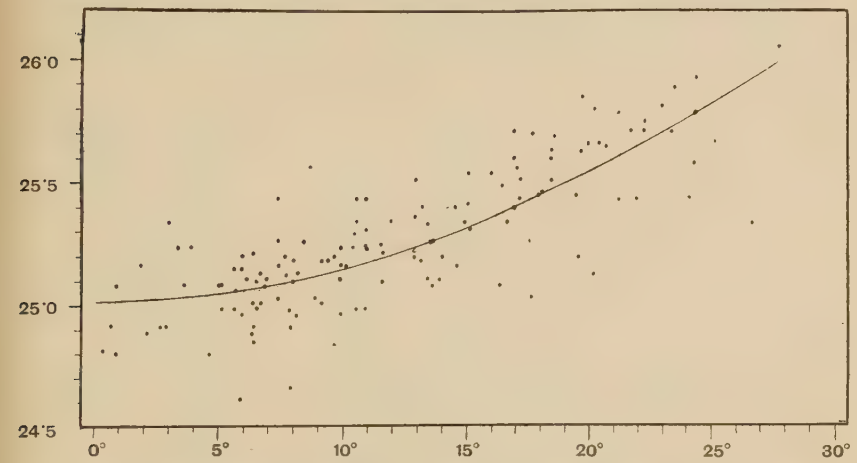


Fig. 1.—Solar Rotation Period from Long-lived Spots—Cycle IV. (1913–1923). Abscissæ—solar latitude; Ordinates—sidereal rotation period in days.

position after its reappearance. If the change of longitude be $x°$ in the interval of y days, then the angular movement of the spot is $x°/y$ per mean solar day relative to a meridian on the Sun having a sidereal rotation of period 25·38 days, this being the period adopted in the *Nautical Almanac* for the computation of the longitude of the central meridian as seen from the Earth. Thus the sidereal angular motion of the spot per mean solar day is $\frac{360°}{25·38}+\frac{x°}{y}$, and the corresponding period is $360/\left(\frac{360}{25·38}+\frac{x}{y}\right)$ days.

3. Altogether 449 spots have been considered. They range in latitude from the Equator to 30°, but there are relatively few above 25°. Of the total number, only eight spots could be traced as the following components of groups. The results for the individual spots in Cycle IV. (1913–1923) are given in the diagram, in which the periods are plotted against the latitudes.

From the diagram it is seen that there is a considerable dispersion in the results as given by individual spots. This is true of all determinations of the solar rotation from spots. It is not due to error of measurement, but to differences in the movement of different spots. The dispersion arises in part from the fact that the motion of the spot is not uniform. When a spot develops as the leader of a group it moves forward in longitude, whilst increasing in size, and then apparently moves in the opposite direction when diminishing. Thus the period of rotation derived from such a spot depends partly on the phase of the development of the two parts compared. The motions in longitude of the principal components of groups are now being investigated.

The following table gives the results obtained from the four cycles for each 5° of latitude from the Equator to 30°. The mean latitude (ϕ), the sidereal diurnal motion (ξ), and the number of spots (n) are given in each case.

TABLE I.

		Cycle I.	II.	III.	IV.	Mean.	Observed *minus* Computed.
0°–5°	ϕ	2°·5	2°·2	3°·0	2°·6	2°·6	
	ξ	**14°·34**	**14°·46**	**14°·39**	**14°·38**	**14°·39**	+°·02
	n	7	7	12	14	40	
5°–10°	ϕ	8°·3	7°·5	7°·4	7°·4	7°·6	
	ξ	**14°·29**	**14°·30**	**14°·31**	**14°·36**	**14°·33**	°·00
	n	22	22	20	47	111	
10°–15°	ϕ	12°·8	12°·3	12°·8	12°·4	12°·5	
	ξ	**14°·23**	**14°·23**	**14°·27**	**14°·25**	**14°·25**	°·00
	n	23	47	34	32	136	
15°–20°	ϕ	17°·0	17°·9	16°·9	17°·6	17°·4	
	ξ	**14°·16**	**14°·12**	**14°·15**	**14°·12**	**14°·13**	−°·01
	n	18	27	24	26	95	
20°–25°	ϕ	22°·2	21°·8	22°·1	22°·4	22°·2	
	ξ	**14°·00**	**14°·02**	**13°·99**	**14°·03**	**14°·01**	+°·01
	n	15	14	7	18	54	
25°–30°	ϕ	25°·9	28°·4	27°·8	26°·8	27°·3	
	ξ	**13°·87**	**13°·78**	**13°·78**	**14°·01**	**13°·85**	+°·02
	n	4	5	2	2	13	

A comparison of the rotation periods from these figures for the four cycles is shown in fig. 2. To avoid confusion, the curves have been displaced relatively to one another by 0·2 days.

It will be seen that the values for the separate cycles are in close accordance. As far as this type of sunspot is concerned, there is no difference in the law of rotation from cycle to cycle.

The value of the sidereal angular daily motion found from the data given in Table I. is

$$\xi = 14°{\cdot}37 - 2°{\cdot}60 \sin^2 \phi.$$

The discordances from this formula are given in the last column of the table.

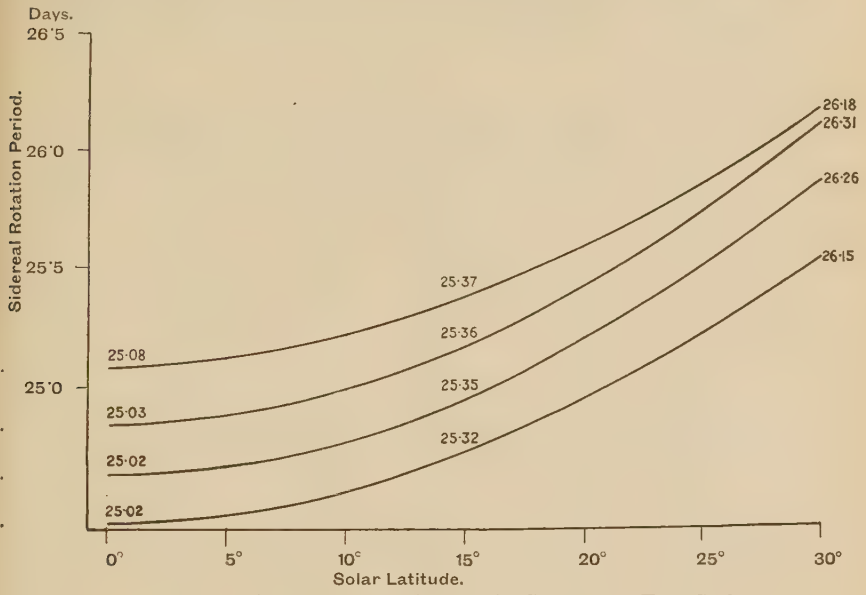


FIG. 2.—Comparison of Rotation Periods given by Sunspots in Four Cycles (1878–1923).

The mean deviation of a single spot from this formula is

$$\pm 0°{\cdot}08,$$

and is the same in all latitudes.

4. A comparison of the results obtained by Carrington, Spoerer, and Mr. and Mrs. Maunder is given in Table II.

TABLE II.

Sidereal Angular Daily Motion.

Latitude.	Spoerer.	Carrington.	Maunder.	Greenwich.
°	°	°	°	°
0	14·35	14·42	14·44	14·37
5	14·32	14·38	14·43	14·35
10	14·26	14·29	14·38	14·29
15	14·15	14·16	14·30	14·20
20	14·00	14·00	14·19	14·07
25	13·80	13·81	14·06	13·91
30	13·57	13·60	13·91	13·72
35	13·30	13·38	13·74	(13·53)

5. It was of interest to see whether the alternation in polarity of the spots discovered by the Mount Wilson observers made any difference. Cycles I. and III. were grouped together for the N. and S. hemispheres separately, and Cycles II. and IV. similarly. The following table, applicable to the preceding components of bipolar groups, shows that the change of polarity makes no difference in the derived rotation periods.

TABLE III.

		Northern Hemisphere.		Southern Hemisphere.		Mean.
		Cycles I. and III.	Cycles II. and IV.	Cycles I. and III.	Cycles II. and IV.	
0°–5°	ϕ	2°·5	2°·3	3°·0	2°·7	2°·6
	ξ	**14°·35**	**14°·39**	**14°·38**	**14°·45**	**14°·39**
	n	7	13	12	8	40
5°–10°	ϕ	8°·1	7°·7	7°·8	7°·2	7°·6
	ξ	**14°·34**	**14°·33**	**14°·28**	**14°·35**	**14°·33**
	n	15	32	27	37	111
10°–15°	ϕ	13°·0	12°·1	12°·7	12°·7	12°·5
	ξ	**14°·26**	**14°·23**	**14°·25**	**14°·25**	**14°·25**
	n	29	48	28	31	136
15°–20°	ϕ	17°·1	17°·8	16°·8	17°·6	17°·4
	ξ	**14°·14**	**14°·12**	**14°·16**	**14°·12**	**14°·13**
	n	14	31	28	22	95
20°–25°	ϕ	22°·0	22°·5	22°·4	21°·9	22°·2
	ξ	**14°·02**	**14°·06**	**13°·96**	**13°·99**	**14°·01**
	n	13	15	9	17	54
25°–30°	ϕ	27°·3	26°·6	25°·7	28°·9	27°·3
	ξ	**13°·85**	**13°·98**	**13°·83**	**13°·75**	**13°·85**
	n	3	3	3	4	13

6. Finally, the observations were classified according to the hemisphere, N. or S., and tabulated as before in zones of every 5° in latitude. The two formulæ derived from the means are as follows :—

Northern hemisphere, No. of spots 223 . $\xi = 14°·36 - 2°·34 \sin^2 \phi$.
Southern hemisphere, No. of spots 226 . $\xi = 14°·38 - 2°·82 \sin^2 \phi$.

The results for the two hemispheres are in good agreement, and do not point to any measurable difference in the law of rotation of the two hemispheres from the Equator to ±30°.

It is proposed to give in detail the observations on which the discussion is based in the Greenwich volume for 1924.

Royal Observatory, Greenwich :
1925 *March* 10.

Characteristic Movements of Sunspots.

(*Communicated by the Astronomer Royal.*)

In the *Monthly Notices* for 1919 April, Mr. Maunder gave a number of diagrams to illustrate the typical movements of the components of sunspot groups. His conclusions are summed up (p. 457) as follows :—

" An examination of these diagrams shows that a remarkable action takes place in the early history of several of the principal groups. Two chief spots become distinguishable while the group is still small, of which the one preceding in longitude—the 'leader'—moves forward in longitude for several successive days, but with diminishing speed. The rear spot, on the contrary, retrogrades, or remains stationary in longitude ; so that the distance between these two spots increases, and the group lengthens out. The average length of a fully developed group of this type is from 10° to 12°. The leader spot increases in area and advances in longitude for a few days, the two changes, in most cases, coming to an end about the same time. . . . After the halt of the leader the decay of the group sets in ; but the process is much slower than the growth. The members of the group disappear with the exception of the leader, which generally becomes circular and strongly defined, stable in area, and with a tendency to a slow retrogression in longitude."

In the present paper all recurrent spots seen from their formation in the four cycles since 1878 have been included, as it seemed desirable that a more complete examination of these characteristic movements should be made, together with a definitive determination of their amounts.

It was found at the outset that practically all the data came from the leading spots of " streams " or bipolar groups. Apparently only about 5 per cent. of recurrent groups reappear with the following components, which are essentially much shorter-lived than the leaders. The major part of this discussion relates, therefore, to the leaders of groups, but the followers are dealt with as fully as possible in Table VI.

The longitudes of the leader spots considered were arranged for each day, beginning with the day on which the spot appeared. [Spots which commenced when they were invisible from the Earth have not been included in the tables.] These were corrected for the movement appropriate to the latitude by the formula $\xi = 14°{\cdot}37 - 2°{\cdot}60 \sin^2 \phi$, given in the preceding paper. The means being taken out, the displacement in longitude from the mean position is obtained for each day during the life of the spot when it was visible from the Earth. There are of necessity gaps of about 14 days' duration, when the spots were on the invisible hemisphere. For example, the leading spot of Group 8353 (*Greenwich Photoheliographic Results*, 1917, p. D121) was seen for 10 days ; then after an interval of 15 days was seen from the 25th to the 34th day ; after which it disappeared.

This spot contributes the following data. [The measures on the 10th and 25th days have not been included, as the spot was more than 75° from the Sun's central meridian.]

TABLE I.

Day.	Displacement.	Area.	Day.	Displacement.	Area.
	°			°	
1	−4·5	14	26	+1·3	105
2	−4·0	31	27	+0·9	96
3	−2·1	85	28	+1·1	106
4	−0·1	155	29	+0·8	93
5	+0·7	148	30	+0·5	114
6	+1·1	152	31	+0·6	96
7	+1·0	123	32	+0·5	78
8	+1·1	149	33	+0·3	39
9	+0·9	154	34	+0·3	13

A division of the material was made between (*a*) Recurrent spots which were seen to disappear at their first return, (*b*) Spots which passed off the disc after their first return but did not come round again, and (*c*) Spots which returned a second time. The length of life in these three classes is approximately 30, 45, and 60 days respectively.

Examination of the three Tables II., III., and IV., giving collected results, shows that nearly all the spots which were visible during the first three or four days were *together* visible from the 27th to 28th day in class (*a*), for the 28th to 30th day in class (*b*), and from the 26th to 32nd day and again from the 53rd to the 58th day in class (*c*). Thus a comparison of positions during these periods with those in the first few days of the lives of spots is obtained from a mean of about 60, 60, and 30 spots respectively. The means for these days were obtained first. These positions were then used as fiducial points from which mean positions for other days were derived. For example, in Table IV., class (*c*), 30 spots were seen on days 1, 2, 3, 4; 27, 28, 29, 30; 54. The means of these 30 spots were taken, and gave the relative positions as tabulated. But on the 8th day only 14 spots were seen. The difference between the mean position of these 14 spots and the same 14 on the 4th day was +0°·62; this was applied to −0°·11 of 4th day, and gave +0°·51 for the 8th day. Similarly for the 21st day, 14 spots were seen which occur on the 29th day, the difference of the mean position being +0°·47. Applying this to the value +0°·81 already found, +1°·28 was obtained, as given in Table IV.

Actually, values found in this more accurate but slightly complicated manner differ very little from those obtained by taking the simple means of the displacements.

The corresponding mean areas, corrected for foreshortening and expressed in millionths of the Sun's visible hemisphere, were derived in a similar way, and are included in the tables and diagrams for comparison with the displacements in longitude.

TABLE II.

(*a*) 30-*Day* "*Leader*" *Spots.*

Day.	Displacement.	Area.	Number of Spots.	Day.	Displacement.	Area.	Number of Spots.
	°				°		
1	−2·30	106	60	21	+0·95	189	16
2	−1·36	167	60	22	+0·86	163	22
3	−0·60	219	60	23	+0·71	154	27
4	−0·04	243	56	24	+0·72	142	34
5	+0·19	268	49	25	+0·48	131	42
6	+0·27	285	43	26	+0·47	119	50
7	+0·46	301	38	27	+0·51	104	54
8	+0·71	283	29	28	+0·42	99	54
9	+0·94	297	23	29	+0·41	88	51
10	+0·58	280	14	30	+0·27	79	45
				31	+0·19	72	43
				32	+0·19	56	33
				33	+0·19	41	26
				34	+0·13	11	21
				35	+0·22	17	12

TABLE III.

(*b*) 45-*Day* "*Leader*" *Spots.*

Day.	Displacement.	Area.	Number.	Day.	Displacement.	Area.	Number.
	°				°		
1	−2·18	128	58	20	+1·22	239	13
2	−1·03	242	58	21	+0·79	281	20
3	−0·39	306	58	22	+0·60	290	23
4	0·00	340	49	23	+0·63	271	28
5	+0·32	373	47	24	+0·46	247	34
6	+0·54	409	41	25	+0·37	263	39
7	+0·42	436	34	26	+0·28	253	42
8	+0·58	450	28	27	+0·23	246	51
9	+0·33	458	23	28	+0·18	238	58
10	+0·28	452	18	29	+0·18	231	58
11	+0·71	394	14	30	+0·18	216	58
				31	+0·09	208	54
				32	+0·09	165	46
				33	+0·23	177	40
				34	+0·03	157	36
				35	+0·10	144	30
				36	+0·16	131	24
				37	+0·06	99	20
				38	−0·10	107	15

"Leader" Spots (30 and 45 days' duration)—Mean Displacements in Longitude and Areas.

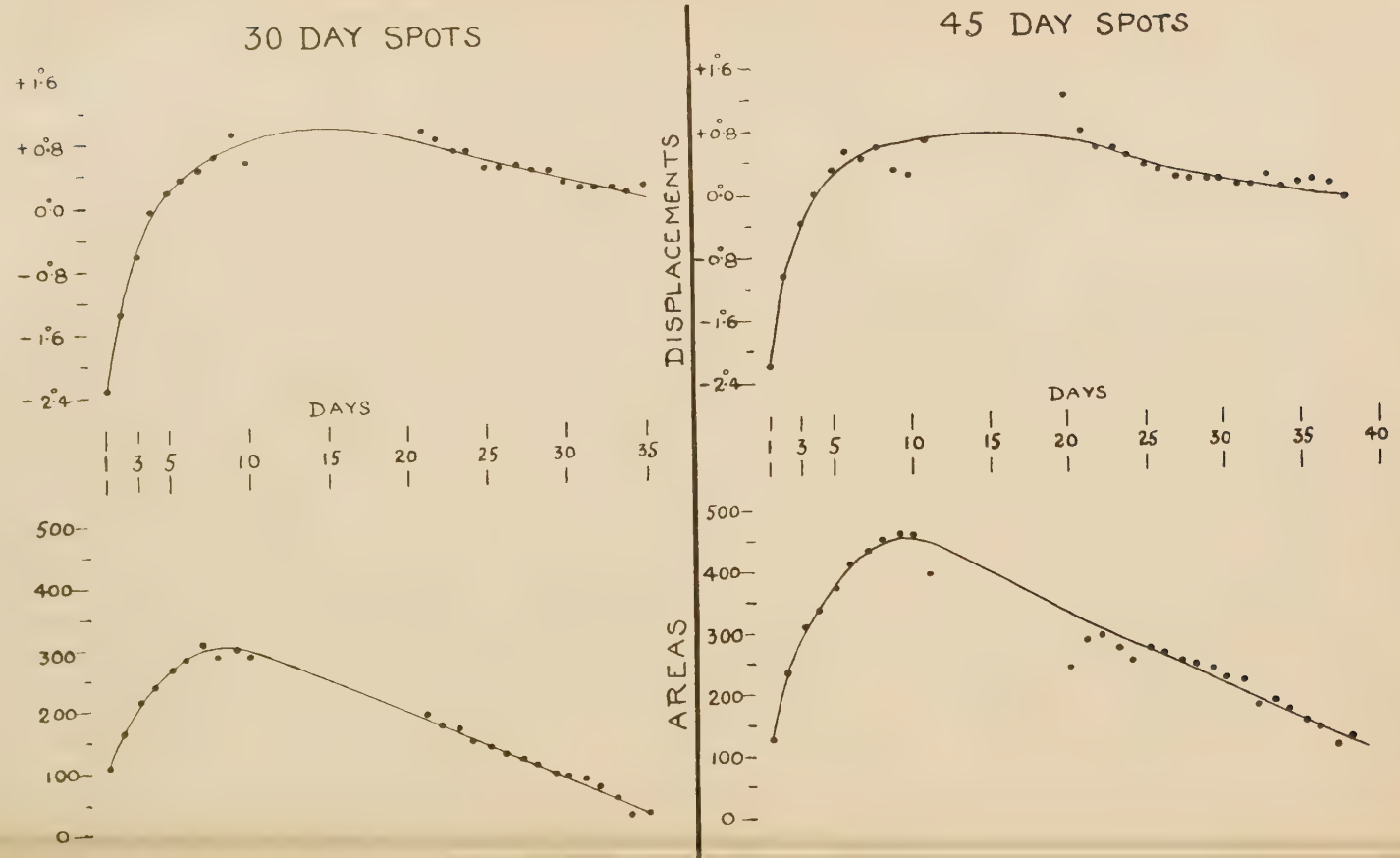


TABLE IV.

(c) 60-*Day* " *Leader* " *Spots.*

Day.	Displacement.	Area.	Number.	Day.	Displacement.	Area.	Number.	Day.	Displacement.	Area.	Number.
	°				°				°		
1	−2·84	119	30	21	+1·28	356	14	48	+0·08	175	10
2	−1·57	230	30	22	+1·34	367	16	49	−0·11	158	15
3	−0·58	308	30	23	+1·19	349	17	50	−0·17	144	17
4	−0·11	345	30	24	+1·01	351	23	51	−0·24	134	22
5	−0·01	385	26	25	+0·94	342	26	52	−0·29	130	24
6	+0·23	415	16	26	+1·05	337	29	53	−0·30	118	27
7	+0·43	428	15	27	+1·04	328	30	54	−0·38	112	30
8	+0·51	485	14	28	+0·93	316	30	55	−0·46	111	29
9	+0·79	550	9	29	+0·81	303	30	56	−0·56	89	27
				30	+0·69	296	30	57	−0·56	81	28
				31	+0·56	296	29	58	−0·63	85	26
				32	+0·65	278	28	59	−0·55	57	21
				33	+0·49	276	17	60	−0·66	43	13
				34	+0·48	259	15	61	−0·98	11	7
				35	+0·43	267	13				

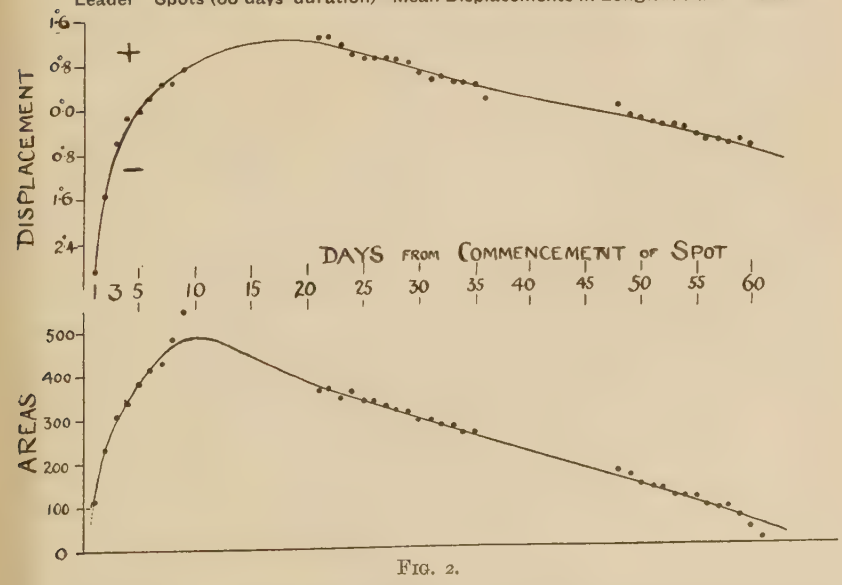


FIG. 2.

From the result of this tabulation we obtain the typical life-history of a " leader " spot of a bipolar group both as regards its motion and area. This is very clearly shown in the accompanying diagrams.

The average diurnal movement of the spots in longitude is derived from the curves as follows :—

TABLE V.

Diurnal Movement in Longitude of "Leader" Spots.

Day.	(*a*).	(*b*).	(*c*).
	°	°	°
1–3	+0·85	+0·90	+1·10
3–5	+0·40	+0·30	+0·40
5–7	+0·15	+0·15	+0·15
7–9	+0·15	+0·10	+0·15
9–21	0·00	0·00	+0·04
21–36	−0·06	−0·05	−0·06
48–61	..	..	−0·05

Thus in the first two days of its life the leader spot of a group appears to be moving rapidly forward in longitude at the rate of about 1°·0 a day. In the next two days the rate has diminished to about 0°·4 per day, and it continues to diminish until about the 15th day, when the spot is moving with the mean velocity corresponding to its latitude. By about the 20th day it is moving slowly backwards at a rate of about 0°·06 per day. Corresponding changes take place in the size of the spots, which increase rapidly at the start, reach a maximum at about the 9th day, and then slowly decline. It may be noticed that the area is a maximum at nearly the same time both for spots which last 60 days and for those of half that duration.

The *following* components of recurrent bipolar groups offer considerably less data than the leaders. They last on the average only for 11 days, or one-quarter of the length of life of the leader. Of the total number of spot groups considered only 8, *i.e.* about 5 per cent., showed a return of the follower, so that the statistics are necessarily confined to such spots seen during a single transit of the Sun's disc. The mean values of diurnal movement given in Table VI. have been obtained from as many "followers" to the leaders included in this discussion as could be used, supplemented by a few others from non-recurrent groups.

TABLE VI.

Diurnal Movement in Longitude of "Follower" Spots.

Day.	Diurnal Movement.
	°
1–3	−0·30
3–5	−0·15
5–7	−0·07
7–11	−0·01

It will be remarked that the movement of the "follower" is consistently slower than the mean velocity corresponding to its latitude, particularly at its formation.

The corresponding changes in area show a sharp rise to a maximum on about the 3rd or 4th day, followed by a steady decline. The majority of these spots, usually composite in character, break up a few days after their maximum, the component parts soon disappearing. The behaviour of these spots contrasts with that of the "leaders," which generally, though not invariably, preserve their entirety and diminish to very small spots before finally disappearing.

Returning to the leader spots, those of classes (*a*) and (*b*) were divided into two groups according to latitude, 0° to 12½° and 12½° to 30°. No difference in behaviour could be detected.

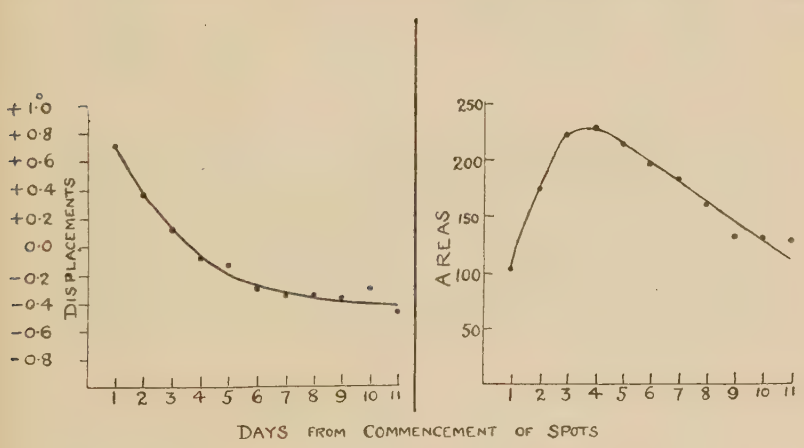


FIG. 3.

They were also arranged according to polarity, *i.e.* spots of the N. hemisphere in Cycles I. and III. were combined with those of the S. hemisphere in Cycles II. and IV., and *vice versa*. A comparison of the deduced rates of the respective movements of spots of "positive" polarity and those of "negative" polarity does not indicate any dependence upon the sign.

TABLE VII.

Day.	Diurnal Movement of *Positive* Spot.	*Negative* Spot.
	°	°
1–3	+·80	+·85
3–5	+·30	+·35
5–7	+·15	+·20
7–9	+·10	+·10
9–21	−·01	+·02
21–35	−·06	−·05

Conclusion.

The most remarkable feature is the rapid forward movement in longitude of a "leader" spot at its formation, accompanied by its increase in area. For the first two days the diurnal motion is +1°·0 greater than the average movement of a spot in the same latitude. At 15° from the Equator this amounts to 15°·2. Comparing this result with sidereal diurnal movements found in other ways, we get

At Latitude 15°

	°
Average spot *	14·2
Reversing layer †	14·3
Faculæ (Greenwich) ‡	14·4
λ4227 †	14·8
H_α †	14·9
Spot at commencement	15·2

If these differences may be correlated with variation in level, a "leader" spot at its formation is high and rapidly descends. At the same time it increases in area.

It may be worth while to examine a "leader" spot when a suitable one appears for velocity in the line of sight, to see what vertical motion accompanies the horizontal motion. Possibly an examination on spectroheliograms of the flocculi surrounding a "leader" spot for a few days after its formation might also throw some light on the matter.

Royal Observatory, Greenwich:
1925 April 3.

On the Cause of Anomalous Determinations of Time.

By Professor R. A. Sampson, F.R.S. (Plates 14, 15.)

With a view to locating the source of the discrepancies in time determination which I pointed out, as between the observatories of Paris and Edinburgh, in 1920,§ and in 1922 showed to be inherent, with varying character, in the time determinations of six of the principal observatories,‖ attention has been given at Edinburgh to improving time determinations in any way that offered, and especially in the clock and chronograph system. Thus the clocks are compared daily by the

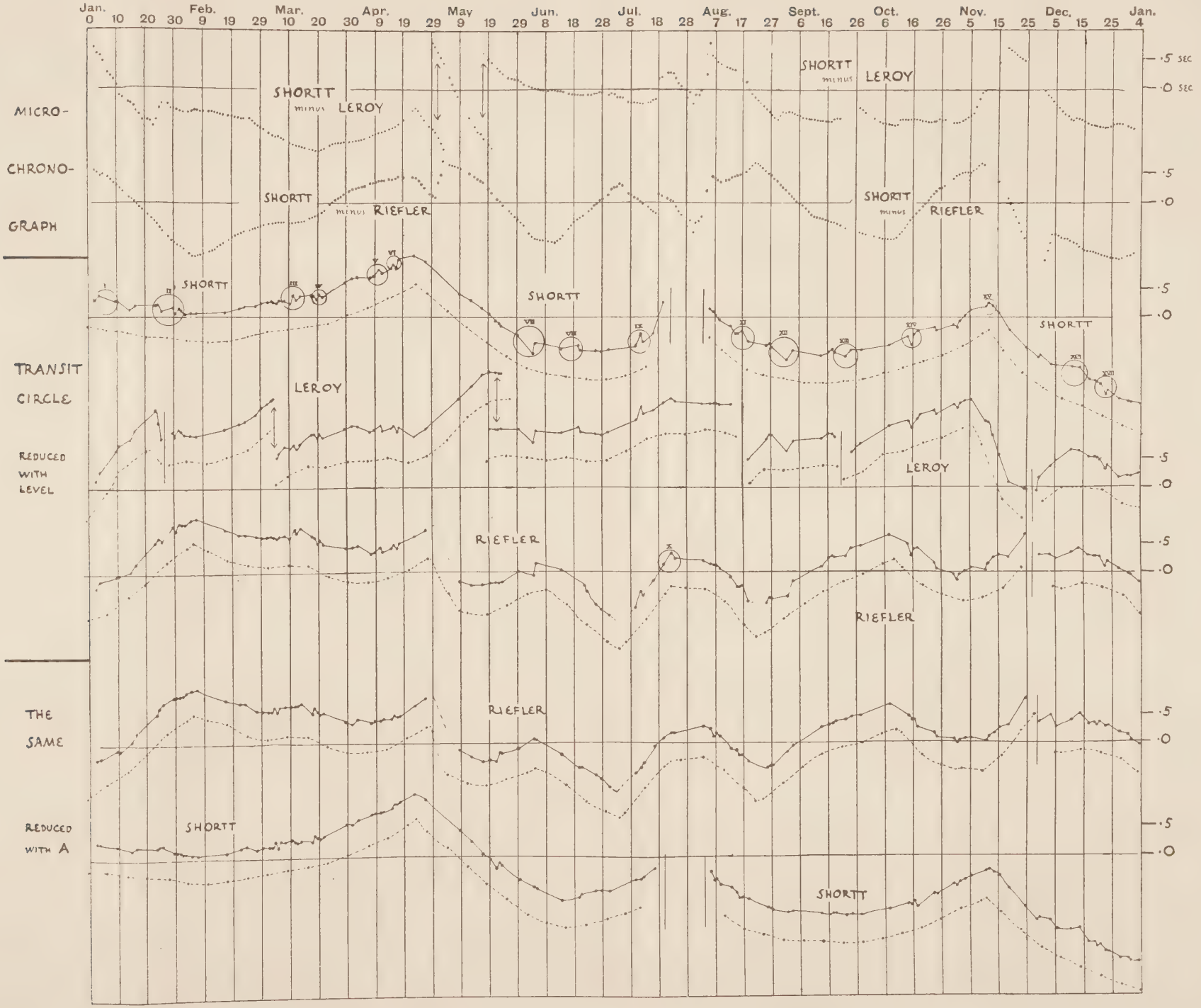
* "The Sun's Rotation Period from Long-Lived Spots," preceding paper, p. 551.

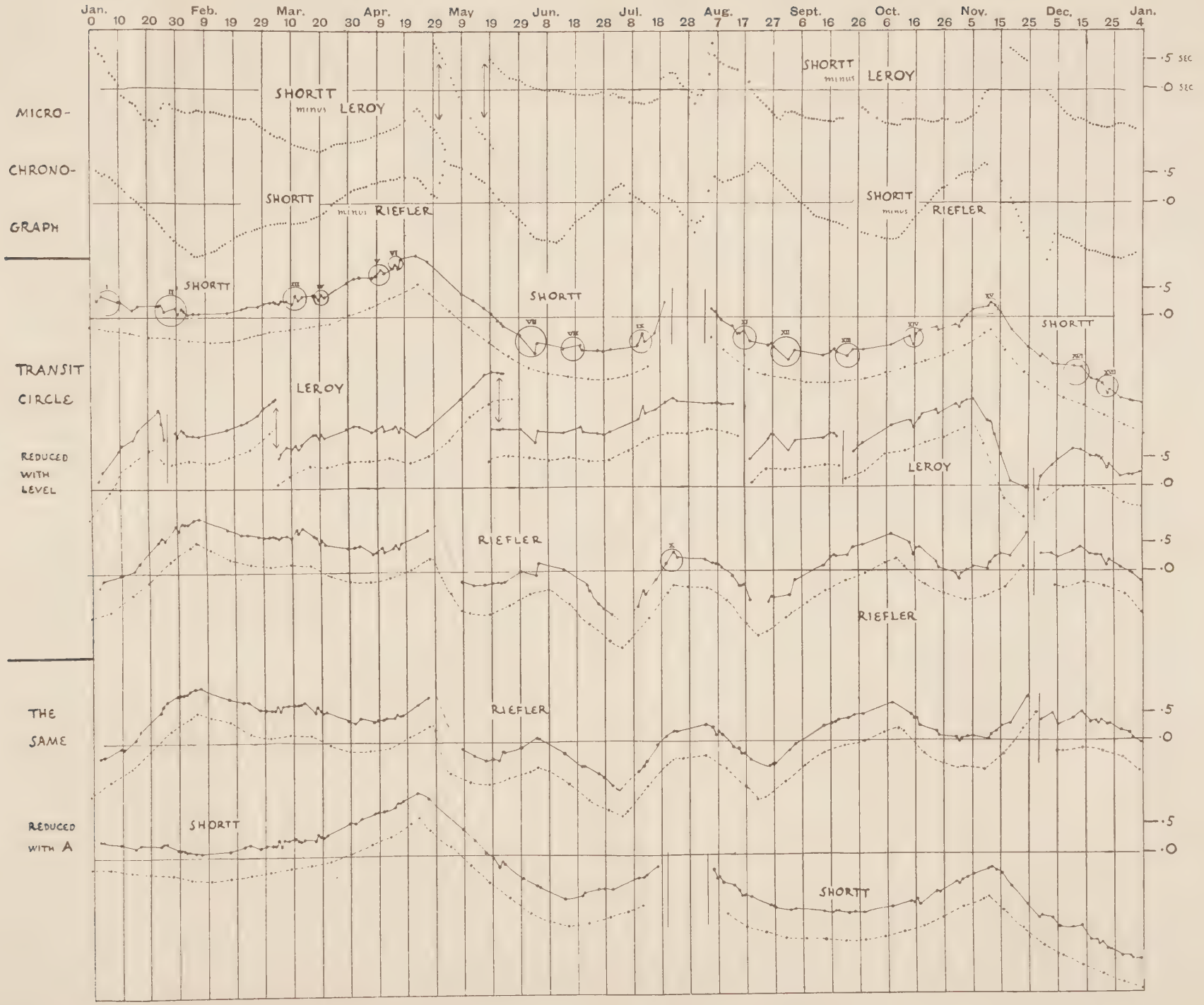
† "Investigation of Rotation Period of the Sun," by W. S. Adams and J. B. Lasby, p. 118.

‡ *M.N.R.A.S.*, **84**, 441.

§ "On Clock Errors and Wireless Time Signals," *M.N.*, **71**, 90.

‖ "On the Determination of Time at Different Observatories," *M.N.*, **72**, 215.


Jan. 0 10 20 30 Feb. 9 19 29 Mar. 10 20 30 Apr. 9 19 29 May 9 19 29 Jun. 8 18 28 Jul. 8 18 28 Aug. 7 17 27 Sept. 6 16 26 Oct. 6 16 26 Nov. 5 15 25 Dec. 5 15 25 Jan. 4
MICRO-
CHRONO-
GRAPH
SHORTT minus LEROY
SHORTT minus RIEFLER
TRANSIT CIRCLE
REDUCED WITH LEVEL
SHORTT
LEROY
RIEFLER
THE SAME
REDUCED WITH A
·5 SEC
·0 SEC
·5
·0


Jan. 0 10 20 30 Feb. 9 19 29 Mar. 10 20 30 Apr. 9 19 29 May 9 19 29 Jun. 8 18 28 Jul. 8 18 28 Aug. 7 17 27 Sept. 6 16 26 Oct. 6 16 26 Nov. 5 15 25 Dec. 5 15 25 Jan. 4
MICRO-
CHRONO-
GRAPH
SHORTT minus LEROY
SHORTT minus RIEFLER
TRANSIT CIRCLE
REDUCED WITH LEVEL
LEROY
RIEFLER
THE SAME
REDUCED WITH A
SHORTT
·5 sec
0 sec
·5
·0

microchronograph,* which excludes all errors in registration, and in the year 1924, to which this paper refers, there were three clocks of the first grade in use—Riefler No. 258, Leroy No. 1230, and the original model of Mr. Shortt's clock. To these has been added in the present year another Shortt clock of a slightly improved pattern—Shortt No. 4. This equipment is not redundant. Certainty over prolonged periods to the hundredth of a second is the requirement. The observed corrections of these clocks are shown on the accompanying plate (Plate 14). The scale of this chart is such that a gradient of 45° implies a rate of ·05 second per day. It is therefore intended to show errors up. Accordingly it will be remarked how many accidents have beset the clocks, of leaks in the cases, failure in temperature control, stoppages, changes of rate, either by calculated barometric change or without assignable cause. When any such accident supervenes upon a clock, that clock is for the time being out of count for discriminating between different classes of discrepancies shown in the transit-circle time observations. It is only by possessing a battery of evidence in reserve that certainty can be reached on this point.

It happens that at this observatory, in the year 1924, the cases of anomaly in clock error were both numerous and well marked. The evidence assembled in Plate 14 illustrates the method by which time work is here discussed.

The two upper traces show the relative errors of the three clocks, recorded by the microchronograph. Their general smoothness will be appreciated at once. At the same time they are far from being straight for prolonged periods. It is not necessary to particularise the causes of all the breaks that are noted. The abrupt changes of gradient are, as a rule, due to barometric changes, deliberately introduced in order to alter the rate. For determination of the absolute errors of the clocks the relative errors here recorded are taken as fiducial data. Even on the evidence of these charts, in which the scale of representation is reduced and the actual measures are rounded off from the thousandths to the hundredths of a second, it will be readily conceded that where the trace of two clocks appears smooth, anything like an abrupt change of error in either one or the other of them, to the amount of a few hundredths of a second, is excluded, and must be attributed to the source that avers it.

Now consider the transit-circle observations for clock correction. The usual observation made here consists of a measurement of collimation error, upon the north and south collimators, an observation of level, with the mercury bath and Bohnenberger eyepiece before and again at the conclusion of the series, two polar stars, and ten time stars. The tabular places of the stars are uniformly referred to Boss's P.G.C. The observations are made with the clock Leroy marking seconds on a Fuess chronograph. The corrections of Shortt and Riefler are then deduced from the relative data supplied by the microchronograph. From the clocks that keep sidereal time the nutation is removed. The results for the year are then those shown on Plate 14.

* Cf. *M.N.*, **78**, 592.

It will be noted, as has often been remarked before, that this imposes numerous marked cases of simultaneous deviations upon all the clocks. It now remains to assign what is true clock correction and what is transit-circle error. For this purpose, the most reliable clock is taken as "director," in this case the clock Shortt, and an error adopted for it which shall keep as close as may be to the transit-circle record, while taking due note of the microchronograph relative record. The record for the clock Shortt shows hardly any feature except a curvature due to a minute leak which has not been located. The other clocks, though almost equally steady for short periods, are more liable to fluctuations and breaks. The results are shown, as adopted, below the observed corrections of each clock. These adopted traces are therefore a single coherent system, as are also the three observed traces. The values "*Observed—Adopted*" are referred to as the erratics.

There will be noted at once several conspicuous erratics that no one would ascribe to the clocks. Proceeding from these to the less conspicuous cases, many more will be conceded. Without by any means exhausting the evidence, I have marked by a ring seventeen clear cases for examination, with the intention of using them to determine what feature in the observations has introduced them.

In the process of measuring collimation error, the movable wire of the N. collimator is set upon the wire of the S. collimator, which is untouched. We denote this setting by N/S. The collimation line of the transit circle is then set in turn upon the wire of the N. collimator and that of the S. collimator, the measures being denoted by TC/N and TC/S respectively. Further, L being the setting of the collimation line upon its own image in the mercury bath, as habitually, we have

$$\begin{aligned} \text{collimation} &= \textit{constant} - \tfrac{1}{2}[\text{TC/S} + \text{TC/N}] = c \\ \text{level} &= \text{L} - \tfrac{1}{2}[\text{TC/S} + \text{TC/N}] = l, \end{aligned}$$

and besides this we may add

$$\begin{aligned} &\text{azimuth of transit circle east of S. collimator} \\ &= \textit{constant} + \tfrac{1}{2}[\text{TC/N} - \tfrac{1}{2}\text{TC/S}] = \text{A} \\ &\text{azimuth of N. collimator east of S. collimator} \\ &= \textit{constant} - \text{N/S} = [a]. \end{aligned}$$

Since 1913 the third and fourth data have been recorded here, with the view that they might serve as a check on azimuths derived from polars.* These were supposed to be the weak point of the transit observations. There is a proportion of cases when the polars disagree, and, following the formula $m + n \tan \delta$, they might introduce an uncertainty which on occasion could amount to several hundredths of a second in the clock correction. On the other hand, level seemed particularly well determined. The mercury bath is carried between the piers. It is used immediately before and immediately after the observations, but requires to be removed while they are in progress. Hence the two measures are wholly independent. Yet their average

* *Cf.* "The Temperature Coefficients of the Edinburgh Transit Circle," *M.N.*, **75**, 69

range from their mean is only ·01 second, which has to include all progressive changes due to temperature or other causes, and the cases of erratics examined below in Table I offer no suggestion of an unusual feature. Accordingly I have experimented from time to time by employing A to replace the stellar azimuth derived from the formula $a=l \tan \phi - n \sec \phi$ in the reductions, so as to eliminate n, but always with inconclusive results.

In considering the erratics presented by the year 1924, it occurred to me to retain n and use A in order to eliminate l. One thing that suggested this course was the following. In each time set a certain number of high stars are included. If in the formula of correction, $m+n \tan \delta$, the quantity n was at fault, these should stand out. Hardly anything of the kind was shown even when they were grouped for the purpose.

The complete elimination of l from the observations is effected very simply. Deriving n as usual from the polars and time stars, compute

$$a=l \tan \phi - n \sec \phi,$$

and then, noting that

$$m=a \operatorname{cosec} \phi + n \cot \phi,$$

apply to m the correction

$$+(\mathrm{A}-a) \operatorname{cosec} \phi,$$

and the same quantity with the reversed sign to the clock correction. This was done to all the observations; they were marked on a chart as before, using, of course, the same microchronograph differences, and a fresh set of adopted clock errors derived, without any other variation of method.

If now the seventeen dates of occurrence of striking erratics are looked for, fifteen will be found to have disappeared totally, one (No. IV) partially, while one (No. XVI) cannot be said to be improved. At the same time the new reduction introduces some faults of its own at other places. These are of less amount. The mean erratic for the whole year is reduced from ±·04 second to ±·03 second. This is a very large reduction, as the latter has to cover every possible residual source of error. But it is no part of the present argument to prove that the S. collimator supplies a permanent fiducial mark. It was not designed for any such purpose. It appears, as will be shown further below, to have behaved with extraordinary steadiness in the year under review. It does not follow that it would do so another year. If on any occasion the spider thread in its focal plane had moved east or west relative to its object-glass by as much as ·001 inch, it would introduce an erratic in the clock correction of ±0·2 second. It is astonishing that, as will be seen below, it does not appear to have moved so much in the course of the temperature changes of the whole year. But I would repeat that the thesis is, that the erratics originally conspicuous disappear when the level measures are discarded.

For closer examination I append a table giving numerical details regarding these seventeen erratic cases along with their regular neighbours.

TABLE I.

Details of Selected Cases.

Column 1. Number and date.
" 2. Erratic=TC−Adopted, reduced with *l*.
" 3. " " " reduced with A.
" 4, 5, 6. Instrumental corrections to TC.
" 7. A−*a*.
" 8. Range in clock correction permitted by extreme measures of *l*.
" 9. Temperature.

1.		2. Erratics. With *l*.	3. Erratics. With A.	4. *l*.	5. *n*.	6. *a*.	7. A−*a*.	8. *l* Range from Mean.	9. Temp.
		sec.	sec.	sec.	sec.	sec.	sec.	±sec.	° F.
(I) Jan.	2	*·02*	*·01*	·42	*·03*	·67	·00	·006	44
	3	·09	·00	·39	·00	·58	·08	·015	40
	9	·02	*·01*	·39	*·03*	·62	·03	·012	28
(II) Jan.	24	·07	·07	·43	*·03*	·70	*·03*	·017	..
	25	*·04*	·07	·47	*·06*	·80	*·13*	·017	45
	29	·04	*·01*	·48	·05 *	·61	·03	..	46
	30	*·06*	*·01*	·46	*·03*	·73	*·06*	·003	44
	31	·04	*·01*	·42	*·01*	·64	·04	·003	44
Feb.	1	·01	*·02*	·41	*·02*	·64	·02	·000	41
(III) Mar.	8	·02	·01	·43	*·01*	·64	·02	·012	41
	10	*·05*	·03	·46	*·02*	·70	*·05*	·003	43
	11	·08	*·01*	·43	·05	·55	·09	·003	41
	12	*·02*	*·03*	·44	·03	·61	·03	·020	45
	13	·05	*·01*	·41	·02	·57	·08	·017	45
(IV) Mar.	18	·02	·05	·40	·02	·57	*·01*	·003	39
	19	*·06*	·00	·39	*·03*	·63	*·03*	·003	38
	20	·03	*·04*	·40	·06	·49	·07	·017	39
	21	*·03*	·01	·41	·00	·60	*·01*	·006	38
(V) Apr.	8	·01	·03	·39	*·03*	·62	*·03*	·029	44
	10	·08	·03	·36	·02	·50	·05	·008	37
	11	*·02*	·02	·38	*·02*	·59	*·03*	·003	38
(VI) Apr.	14	·01	·00	·36	*·01*	·54	·02	·017	39
	15	*·06*	·04	·38	·00	·57	·01	·026	42
	16	*·01*	·01	·39	*·01*	·59	*·02*	·008	43
	17	*·09*	·02	·38	·02	·52	·04	·038	45
(VII) May	29	·00	*·01*	·34	·05	·41	·11	·006	53
June	3	*·23*	·04	·33	*·07*	·62	*·09*	·006	52
	4	*·03*	·03	·31	·00	·46	·10	·017	53

* One polar only.

TABLE I—*continued.*

1.	2. Erratics. With *l*.	3. Erratics. With A.	4. *l*.	5. *n*.	6. *a*.	7. A–*a*.	8. *l* Range from Mean.	9. Temp.
	sec.	sec.	sec.	sec.	sec.	sec.	±sec.	° F.
(VIII) June 13	*·04*	*·03*	·28	·02	·38	·20	·003	52
19	·07	·02	·25	*·01*	·38	·20	·026	61
20	*·02*	·07	·32	·00	·48	·08	·003	61
(IX) July 9	*·04*	·01	·28	*·07*	·55	*·07*	·000	63
11	·12	*·03*	·23	*·04*	·41	·11	·012	..
12	*·03*	*·03*	·27	*·09*	·57	*·04*	·006	66
(X) July 19	·09	·02	·19	*·06*	·38	·06	·017	58
22	·10	*·02*	·20	*·04*	·35	·14	·012	59
23	·00	*·03*	·21	*·05*	·41	·07	·000	57
(XI) Aug. 15	·02	*·02*	·18	*·07*	·39	*·02*	·012	57
16	·11	·03	·14	*·09*	·37	·00	·012	55
18	·00	·04	·16	*·13*	·48	*·12*	·008	55
(XII) Aug. 27	·01	*·02*	·18	*·13*	·48	*·04*	·003	56
Sept. 1	*·17*	*·01*	·18	*·21*	·65	*·20*	·012	58
3	·03	*·01*	·20	*·11*	·48	*·07*	·003	59
(XIII) Sept. 16	·04	*·04*	·17	*·11*	·45	*·04*	·008	56
17	·09	·01	·17	*·12*	·45	*·03*	·006	56
18	·02	·02	·16	*·13*	·47	*·08*	·012	53
(XIV) Oct. 14	·02	·00	·15	*·11*	·42	·02	·000	49
15	*·14*	*·02*	·20	*·14*	·54	*·09*	·015	51
16	·03	·02	·17	*·11*	·42	·01	·008	50
(XV) Nov. 10	·04	·00	·19	*·09*	·44	·01	·008	44
11	·00	·02	·20	*·07*	·43	·02	·026	45
13	·09	·06	·16	*·09*	·40	·03	..	42
(XVI) Dec. 5	·03	*·02*	·24	*·10*	·52	*·04*	·000	46
10	·11	·08	·21	*·11*	·51	*·04*	·012	49
13	·15	·18	·22	*·13*	·55	*·08*	·006	45
16	·02	*·01*	·22	*·09*	·45	*·02*	·017	42
(XVII) Dec. 20	·02	·05	·24	*·12*	·58	*·07*	..	51
22	*·10*	*·02*	·25	*·15*	·63	*·13*	·000	46
23	*·01*	·00	·23	*·14*	·59	*·07*	·008	48

We have so far dealt directly with the transit-circle observations and their visibly erratic features. These are removed in the smooth adopted curve of clock correction. It may be noted, however, that the two adopted corrections, derived respectively with l and A, are not the same, but present features of fluctuation and semi-systematic difference throughout the year. I turn now to this point.

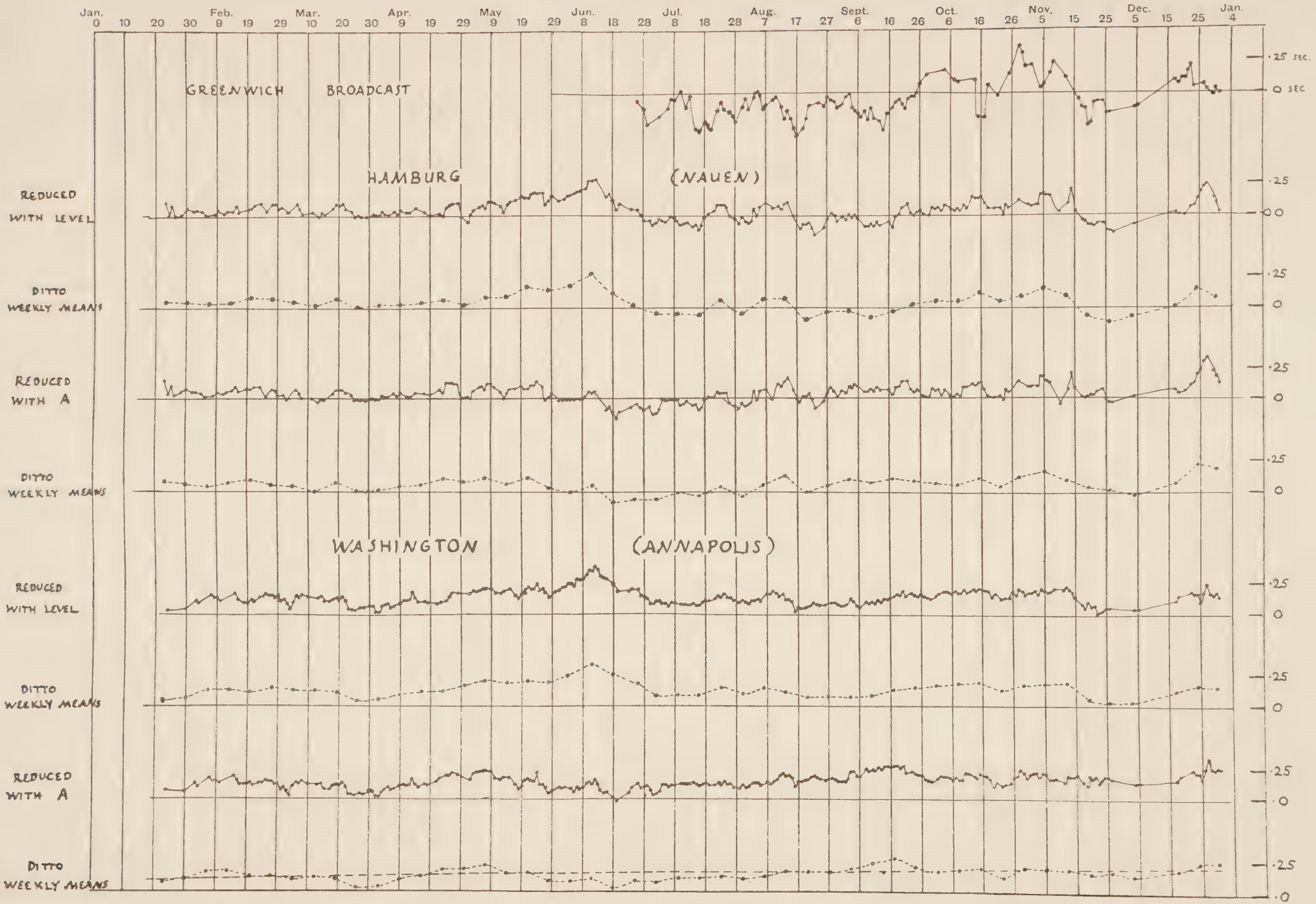
In W.T. signals, each issuing observatory emits its signals at a time provisionally determined, afterwards publishing its definitive time when its clock correction has been adopted. An exception may be found in Greenwich, where the signal relayed by the British Broadcasting Company is not followed by any published corrections. Apart from this case, the final outcome of the receipt of W.T. signals is a comparison of the definitive times derived from the adopted clock corrections of each observatory. One might expect a close agreement, constant corrections of longitude or lag being set aside. But, as I have already remarked, this is not the case. The comparisons made in *M.N.*, **82**, 1922, depended upon uncorrected times, which were the only times available at the moment. The discrepancies may be changed, but are little diminished, when definitive times are employed.*

Since about the end of January 1924, W.T. signals have been regularly recorded automatically here, including those from Washington (Annapolis). I give on Plate 15 the charts for definitive times of receipt of the Annapolis (17^h) and the Nauen (12^h) signals ("Washington slow" and "Hamburg slow"), the final corrections to these signals being available. I give also since June 26 the Greenwich Broadcast Signal. For the Washington and Hamburg times I add the means for each week, without any other smoothing. In the upper curves for Hamburg and Washington, the Edinburgh adopted times have been derived with level, in the old way. It will be noticed that besides resemblances that may or may not be significant, both curves show one very strong deviation which must certainly be due to a peculiarity of the Edinburgh adopted time. But, referring to the adoption in question and the T.C. observations from which it comes, it cannot be admitted that any adjustment is allowable that would remove this deviation.

Next below the comparisons with adopted times derived from T.C. observations reduced into level, I give those where the level is discarded and the T.C. is reduced with A and n. It will be seen that the deviation in question has disappeared. At the same time the mean deviation for Nauen is reduced from $\pm$·058 sec. to $\pm$·045 sec. This is with regard to a medial constant value. For Annapolis the mean deviation is not reduced; for both curves it remains at $\pm$·047 second. The reason of this appears to be that the true medial line is not level throughout the year, but slightly tilted, as shown by the dotted trace on the diagram. A similar tilt, running over the same months, was noted in the Washington times in 1920.† With respect to such a tilted

* It has been pointed out by B. Wanach (*Ast. Nach.*, No. 5199) that in the paper just referred to the times ascribed to Berlin are really due to the Hamburg Seewarte.

† Cf. *M.N.*, **82**, 219.


Jan. 0 10 20 30
Feb. 9 19 29
Mar. 10 20 30
Apr. 9 19 29
May 9 19 29
Jun. 8 18 28
Jul. 8 18 28
Aug. 7 17 27
Sept. 6 16 26
Oct. 6 16 26
Nov. 5 15 25
Dec. 5 15 25
Jan. 4
GREENWICH BROADCAST
·25 SEC.
0 SEC.
HAMBURG (NAUEN)
REDUCED WITH LEVEL
DITTO WEEKLY MEANS
REDUCED WITH A
DITTO WEEKLY MEANS
·25
0
WASHINGTON (ANNAPOLIS)
REDUCED WITH LEVEL
DITTO WEEKLY MEANS
REDUCED WITH A
DITTO WEEKLY MEANS
·25
0
·0

medial the average discrepancy is ±·037 second. In any case the resultant comparison curve could hardly be more satisfactory, considering that it contains all errors incident to the Washington adoption as well as those of Edinburgh. But a general reduction is not the point. The matter for remark is that the level alone is responsible for a large semi-systematic and persistent discrepancy.

I do not at present enter upon any discussion of the way in which the use of the level introduces these anomalies. I am well aware of the difficulties. There is no method more firmly built into fundamental astronomy than the use of the normal to the geoid, found with the spirit-level or the mercury bath. Besides the subject of the present paper, it enters all the most refined observations of latitude and declination. None the less, I consider that there is no escape from the conclusion that the level as determined here has introduced these rather gross faults, both erratic and semi-systematic, without bearing any evidence upon its face. It may be that something will be found in the method by which level is habitually found, as it is found here, that will explain the anomaly. I cannot say that I see any probability of that, and if not, the subject becomes even more interesting.

I would add some remarks which might seem superfluous but for the position which may be taken by those astronomers who regard time questions as having no bearing of importance upon astronomy proper, inasmuch as time merely measures the phase of the Earth's rotation, and this is eliminated from permanent records. This is a complete misapprehension. The hour hands of the clocks may be regarded as mechanical devices for reproducing for continuous reference the positions of the First Point of Aries (without the nutation) or the Mean Sun, as the case may be. The clocks for right ascensions correspond to the vertical circle of the instrument for declinations. Precisely the same part which the declination circle plays in determining one co-ordinate of a star is played by the clock in determining the other. The only difference is that on the divided circle the division errors are treated as fixed and the nadir point approximately so, while the corresponding quantities of clock rate and clock error require continual redetermination. In either case, if the index error is incorrectly assigned, the deficiency is debited to the co-ordinate of the star, and appears tacitly and automatically in the star ledger. If the fault consisted entirely of what I have called erratics, and if the observations are merely differential, as is commonly assumed to be the case, this would have little effect upon the outcome, except a general deterioration of the material, in respect to its internal consistency. The case is far otherwise when there is a systematic deviation and when we are dealing with fundamental right ascensions. Especially if it appears that such a deviation may be of seasonal character, no single observatory can effectively clear the error from its own observations, and it will remain as a systematic difference between the catalogues it prepares and those prepared elsewhere. If these systematic differences in catalogues are changed from one epoch to another, their differences are transferred to the proper motions of the stars and may distort some of

the most comprehensive and delicate conclusions drawn by astronomy. Such was the position between different observatories until recently, and so it remains in regard to declinations. But now in respect to right ascensions, wireless time signals permit a continuous and searching comparison of the datum adopted every day, with that adopted at other observatories. The fact that this comparison deals with the general adopted index errors for whole groups of fundamental stars, which may be in widely different regions of the sky, and not with the co-ordinates of individual stars, should surely be held to enhance its significance.

Method of Variation of Axes, Time, and Scale.

By T. C. Hudson.

In a previous paper (*M.N.R.A.S.*, **84**, 589) the directed differential equation of the second order

$$^2\mathrm{R}R^2=\dot{\rho}=-r$$

was exhibited as a statement of Newton's law of gravitation.

It may be written in a binary form :

$$\mathrm{F}=0,\ \left(1+R^3\left(\frac{d}{dt}\right)^2\right)\mathrm{R}=\mathrm{F}.$$

The right member of F=0 may be regarded as a *negative* idealisation ; as to *non*-departure from the simple law.

Whatever directed value F may possess, if we denote $\mathrm{VR^1R}$ by H and $\mathrm{V^1RH}-r$ by E, then

$$\mathrm{EH}=\mathrm{HV^1RH}-r\mathrm{VR^1R}=0-0=0,$$

the reason, of course, being that the relation between H and E is that of perpendicularity.

Now, so long as F=0, we find $\Delta\mathrm{H}=0$ and $\Delta\mathrm{E}=0$; and conversely, if $\Delta\mathrm{H}=0$ and $\Delta\mathrm{E}=0$, we have F=0. Thus the negation as to F may be translated into negation as to both H and E, without gain and without loss of import. The import consists in the *invariableness* of

$$\left\{\begin{array}{l} h \text{ planarity ;} \\ \frac{1}{2}H \text{ temporal variation of area (angular momentum) ;} \end{array}\right.$$

$$\left\{\begin{array}{l} e \text{ perihelion ;} \\ E \text{ eccentricity.} \end{array}\right.$$

H and E contain three numbers each as defining-attributes possessing a quantitative nature ; but there is a relation between them (EH=0). Thus there are $2\times3-1$ numerical independencies, whatever system of reference is adopted. Of these 5, the lengths of H and E require 2. The other 3 belong to h and e. It takes two numbers to define h ; the remaining number defines the half-plane which contains e.

This essential triplicity is precisely the triplicity (1) in a single directed quantity, say $\alpha\gamma$, in which α is a directivity and γ is numerical, or (2) in an operator defined by $\alpha\gamma$, namely, $\epsilon v\alpha\gamma$ (see p. 588). This is the operator of finite turn ; about α as axis ; through γ as angle.

If to this "rigid system," h, e, there be supplied some third directivity, say at right angles to both, say $\mathrm{V}he=veh$, then it is readily accepted as a matter of ordinary experience, and conditionally verified as a matter of analysis, that $\mathrm{V}he$ may be turned with them without impairing the relations. And the same holds for any number of them, and any aliquot parts ; and so on. In particular it holds for

$$r=Ce+S\mathrm{V}he$$
$$=(C+\mathrm{VS})e$$

in which

$$C^2+S^2=1,\quad C=\cos\theta,\quad S=\sin\theta$$
$$\mathrm{S}=sS=hS=h\sin\theta=\sin h\theta,$$

the concatenation being merely that of manifold identity.

r may be written also as

$$(\mathrm{o}+\epsilon\mathrm{V}h\theta)e.$$

We have just seen that we can, analytically, turn h, e and $\epsilon\mathrm{V}h\theta e=r$ by one operator τ ; the interrelations are not affected by τ ; θ persists as θ. Thus, if

$$\tau h=h_0 \text{ and } \tau e=e_0 \text{ and } \tau r=r_0,$$

the r_0 is necessarily $\epsilon\mathrm{V}h_0\theta e_0$.

This triplex operator τ can be varied at will, and it can always be so chosen as to turn h and e into h_0 and e_0. Hence h_0 and e_0 are entirely free from obligations, and can be treated as fixed directivities for reference.

Again, the number θ presents no difficulty. Thus, r continually points from the Sun towards the asteroid, and in so doing dictates (with the aid of R) the positions of h and e, while at the same time h_0 and e_0 are fixed and so dictate the plane of r_0, while θ dictates the position in that plane. Thus, while r is r, r_0 indicates the true longitude from perihelion as exhibited on a fixed plane

$$\left(\frac{\mathrm{o}e_0}{\mathrm{o}}+\frac{\mathrm{o}\mathrm{V}h_0e_0}{\mathrm{o}}\right)$$

and measured from a fixed datum e_0.

It is convenient, but not necessary, to postulate that h happened to coincide with h_0 at some instant or other ; that at the same instant e happened to coincide with e_0 and r with r_0. Also, with regard to length, that R, H, E were R_0, H_0, E_0.

An additional demand, for the purpose of completing the postulation, is necessary, and requires care. There was coincidence in the case of the directivities r and r_0 and equality in the case of the numbers R and R_0. What became of the coincidence at the next instant ? This is a question on the answer to which a good deal depends. Did the path of the end of R cut that of R_0 or touch at two consecutive

points ; or more than two ? And were the directed velocities ${}^1\mathrm{R}$, ${}^1\mathrm{R}_0$ identical ? Certainly the element of movement $d\mathrm{R}_0$ should have been *along* $d\mathrm{R}$; but should the two points be moving at the same speed ? In what does verisimilitude consist ? Does it reside especially in the matter of periodic time ? The apses e and $\mathrm{V}h\mathrm{V}he$ are moving.

Should periodic time take account of de or should it be independent of de ? If the former, how is the motion of e to come in ? For, after all, the estimate of periodic time rests on observations, and while it is far from easy to determine heliocentric longitude, the determination of perihelion is much more difficult, and even elusive. On the other hand, if periodic time is defined independently of de, how can h and e be adopted as convenient axes of reference, a use for which they seem to be made ?

The above paragraph is not at all a criticism of classical procedure in the case of the eight major planets. The methods applicable to them are obviously not practicable in the case of the known minor planets as a complete system. Some departure from classical methods is necessitated ; in what direction is departure advisable ?

The evidence, so far as I understand it, seems to point in one general direction (in this connection), and unmistakably in that one direction only, and it is to the effect that the dispute should be submitted to arbitration ; the two referees being E and r, with powers, in the event of their failing to come to an agreement, to appeal to H.

Meanwhile the form of the subjects of integration indicates quite clearly that there are four instants, analogous to four quarter-days in the year, which are especially suitable for settling the accounts of summation. Those instants are defined by the equation

$$\epsilon\mathrm{V}h4\theta=1.$$

At these instants the values and variations of $C=er$ and $S=h\mathrm{V}er$ are especially convenient :

$2\theta/\pi$	0	1	2	3
er	+1	0	−1	0
$h\mathrm{V}er$	0	+1	0	−1

The nature of the case suggests a periodic readjustment of the co-factors of the sine and the cosine of longitude from perihelion at the instants when the sine and the cosine were respectively =0. [Concerning time-grace, etc., see the λ_1, λ_2, λ_3 in the equation on p. 592.]

Suitable occasions for periodic adjustment of various parameters have thus been suggested. It remains to formulate a method for the intermediate procedure, throughout the intervals between those occasions.

Let the r_0 as defined by

$$r_0=\epsilon\mathrm{V}h_0\theta e_0$$

arrive at θ when the time is t_0, moving subject to the conditions

$$d\mathrm{H}_0=0,\ d\mathrm{E}_0=0,$$

$$\mathrm{V}\mathrm{R}_0\left(\frac{d}{dt_0}\right)\mathrm{R}_0=\mathrm{H}_0,\ \upsilon\mathrm{H}_0\left(\frac{d}{dt_0}\right)\mathrm{R}_0=r_0+\mathrm{E}_0.$$

This number t_0, representing time, will not in general be equal to t. We shall require to keep account of $t-t_0$, either by continuous or by repeated estimations of $\log \frac{dt}{dt_0}$. We shall also require $\log R-\log R_0$. And of course θ. And it is essential to evaluate H and E.

Let the projections of $\mathrm{F}H^{-2}$ in the directions corresponding to true longitude from perihelion

0	$\pi/2$	$\pi/2+\theta$
M_0	M_1	CM_1-SM_0,

be

and let the numbers N_1, N_2, N_3 be given by

$$\begin{aligned} dN_1&=2(CM_1-SM_0)d\theta \\ (1+E_0C)N_2&=C\psi \\ d\psi&=(M_1(1+EC)+(CM_1-SM_0)(C+E))d\theta \\ EN_3&=2(-M_0(1+EC)+(CM_1-SM_0)S). \end{aligned}$$

Then, to first order at least,

$$\log R-\log R_0=\log (1+N_1)-\log (1+N_2)$$

and

$$\begin{aligned} 2\log (dt_0/dt)=&4\log (1+N_2) \\ &-3\log (1+N_1) \\ &-\log (1+N_3). \end{aligned}$$

These values resulted from the equations

$$d\mathrm{H}=\mathrm{V}r\mathrm{F}H^{-1}|dr|$$

and

$$d\mathrm{E}=vh\mathrm{F}R^{-1}|dr|+\mathrm{V}d\mathrm{R}\mathrm{V}r\mathrm{F}R^{-2}.$$

On Sir Isaac Newton's Formula for the Attraction of a Spheroid on a Point of its Axis. By Professor A. N. Kriloff, Member of the Russian Academy of Sciences.

The attraction of a spheroid on a point of its axis is given in the second corollary to Proposition XCI. of the first book of the *Principia*. It is expressed in a most peculiar form, and no calculation is given of how this formula could be arrived at by the mathematical processes briefly sketched in Lemma II. of the second book.

It may therefore be of some interest to establish Sir Isaac Newton's formula, using for the sake of clearness modern notation, but preserving his methods, which were afterwards published in his treatise on *Quadratures*. This will give us a slight insight of how far Newton had mastered the "Calculus" even at the time when the *Principia* was written.

In Corollary 1 of the Proposition XC. it is shown that the attraction

of a circle on a corpuscle P on its axis (*i.e.* the straight line passing through its centre and normal to its plane) is proportional to

$$1-\frac{PA}{PH},$$

where A is the centre of the circle and H a point on its circumference.

This being granted, Proposition XCI. runs as follows :

To find the attraction of a corpuscle lying on the axis of a solid of revolution to every point of which are directed equal centripetal forces decreasing in any ratio of the distances.

" Let the corpuscle P, situated on the axis AB of the solid DECG, be

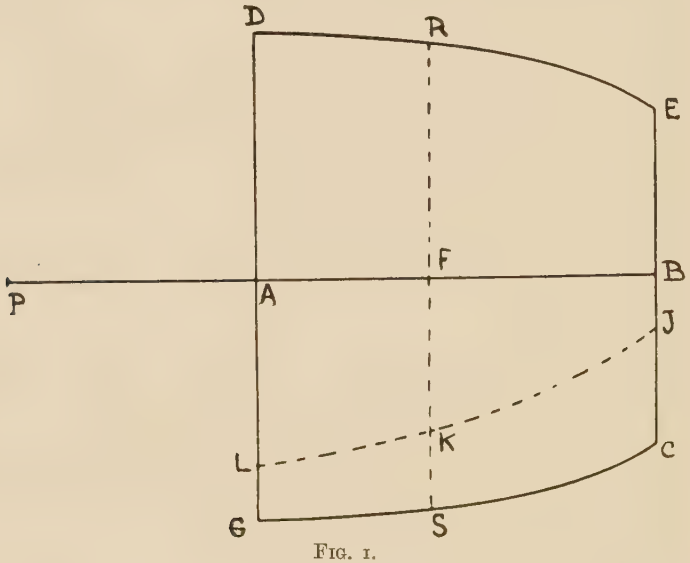


FIG. 1.

attracted by it. Let RFS be any circular section of the solid by a plane perpendicular to its axis ; on the radius FS of this circle situated in a plane PALKB, drawn through the axis, a length FK is taken proportional to the force with which the corpuscle P is attracted by the circle, the point K will describe a curve LKJ, cutting the end planes of the solid in L and J ; the attraction of the corpuscle P by the solid will be proportional to the area LABJ " (fig. 1).

The Corollary 2 to this general proposition is expressed as follows :

" *Cor.* 2.—Thus the force may be found, with which a spheroid AGBC attracts an external corpuscle P lying on its axis AB. Let NKRM be that conic section of which the ordinate ER perpendicular to PE is always equal to the length PD, drawn to the point D where this ordinate cuts the spheroid itself. At the vertices A and B of the spheroid perpendiculars AK and BM to the axis AB are to be erected respectively equal to AP and BP, meeting the conic section in K and M.

Join KM, cutting off the segment KMRK. Let S be the centre of the spheroid and its greatest radius SC, the force with which the spheroid attracts the corpuscle P will be to the force, with which the same corpuscle is attracted by a sphere described on AB as diameter, in the ratio

$$\frac{AS \cdot CS^2 - PS \cdot KMRK}{PS^2 + CS^2 - AS^2} : \frac{AS^3}{3PS^2}.$$

And by a like manner of computation the attraction of the segments of a spheroid can be found (fig. 2)."

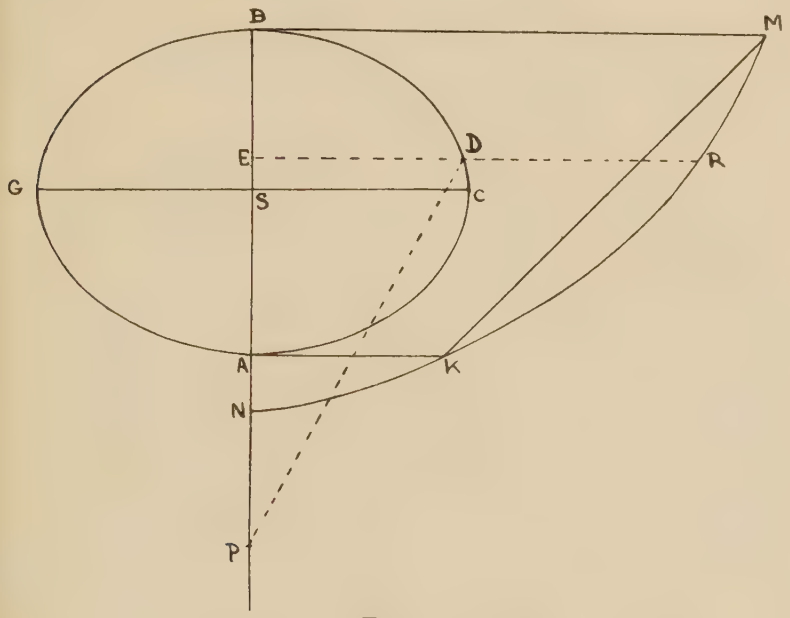


FIG. 2.

Usually, in commenting on Newton, it is shown that this formula gives the same result as the easy integration by modern means; the aim of this paper is to show how naturally this formula is obtained if we follow not the modern but Newton's methods of quadratures.

Let us assume the density of the attracting body to be unity, and the product of the mass of the corpuscle by the coefficient of attraction k, and put for brevity

$$SA=SB=a\,;\ SC=b\,;\ PS=l\,;\ PA=l-a\,;\ PE=x\,;\ PD=X.$$

By the equation of the spheroid we have

$$X^2=Ax^2+2Bx+C,$$

where

$$A=\frac{a^2-b^2}{a^2}\,;\ B=\frac{b^2 l}{a^2}\,;\ C=\frac{b^2}{a^2}(a^2-l^2),$$

and this is the equation of the conic NKRM mentioned in Newton's proposition.

The attraction of the ellipsoid is according to the general theorem and the expression for the attraction of a circle obviously given by the formula

$$F=2\pi k\int_{l-a}^{l+a}\left[1-\frac{x}{X}\right]dx=4\pi k\left[a-\tfrac{1}{2}\int_{l-a}^{l+a}\frac{x}{X}dx\right],$$

and is thus reduced to the calculation of the integral

$$\int_{l-a}^{l+a}\frac{x}{X}\,.\,dx=\int_{l-a}^{l+a}\frac{x\,dx}{\sqrt{Ax^2+2Bx+C}}.$$

Newton evaluates this integral in terms of

$$S=\int_{l-a}^{l+a}X\,.\,dx$$

because this last one has a simple geometrical meaning, namely, the area AKRMB.

We have

$$\begin{aligned}\int X\,dx&=xX-\int\frac{Ax+B}{X}x\,dx\\&=xX-\int\frac{Ax^2+Bx}{X}dx\\&=xX-\int\frac{X^2-Bx-C}{X}dx\\&=xX-\int X\,dx+\int\frac{Bx+C}{X}\,.\,dx,\end{aligned}$$

hence

$$2\int X\,dx=xX+B\int\frac{x\,dx}{X}+C\int\frac{dx}{X}\quad .\quad .\quad .\quad (1)$$

But

$$\begin{aligned}\int\frac{x\,dx}{X}&=\frac{1}{A}\int\frac{Ax+B}{X}dx-\frac{B}{A}\int\frac{dx}{X}\\&=\frac{1}{A}X-\frac{B}{A}\int\frac{dx}{X}\quad .\quad .\quad .\quad .\quad .\quad (2)\end{aligned}$$

and by elimination of $\int\frac{dx}{X}$ we get

$$\int\frac{x}{X}dx=\frac{2B}{B^2-AC}\int X\,dx-\frac{Bx+C}{B^2-AC}\,.\,X\quad .\quad .\quad .\quad (3)$$

Replacing A, B, C by their values and remarking that for

$x=l-a$ we have $X=l-a$

and for

$x=l+a$ we have $X=l+a$,

we obtain after simple reductions

$$\left[\frac{Bx+C}{B^2-AC}X\right]_{l-a}^{l+a}=\frac{2a(l^2+a^2)}{l^2-a^2+b^2},$$

and

$$F=4\pi k\left[a-\frac{lS}{l^2-a^2+b^2}+\frac{a(l^2+a^2)}{l^2-a^2+b^2}\right]=4\pi k\frac{ab^2-l(S-2al)}{l^2-a^2+b^2},$$

where

$$S=\int_{l-a}^{l+a}X\,dx.$$

But

$$S-2al=MRKAB-ABMK=KMRK=S_1.$$

The attraction of a sphere, described on AB as diameter, is

$$F_0=\frac{4}{3}\frac{k\pi a^3}{l^2},$$

hence we have the proportion

$$F : F_0=\frac{ab^2-lS_1}{l^2-a^2+b^2} : \frac{a^3}{3l^2},$$

which represents Newton's formula with the notation we have adopted.

The conic, to the evaluation of the area of which Newton thus reduces the computation of the attraction of a spheroid, is obviously a hyperbola for a prolate and an ellipse for an oblate spheroid; the expression of the area of a conic was already known, hence he considered the problem as solved, but it may be remarked that in his treatise *De curvatura curvarum*, not only the geometrical but also the analytical expressions of the integrals containing the square root of a quadratic function are to be found. In the *Principia* Newton preferred the geometrical way, which explains the peculiar aspect he has given to his formulæ, at which we arrive by a self-consistent and natural process, if we strictly use his methods.

A New Variable Star in Perseus. By A. Stanley Williams.

B.D.+43° 614 (9·3 mag.).

R.A.=2 h 50 m 37 s	Decl.=+43° 32′·3	(1855).
R.A.=2 53 34	Decl.=+43 43·3	(1900).

The variability of the above star does not seem to have been noticed hitherto. It was first recognised to be variable by the writer in January last, when comparing some photographs taken more than twenty years ago with a 4·4-inch portrait lens, and visual observations were commenced at the first opportunity with a 6½-inch reflector. These confirm the variability of the star.

The following estimations of the brightness of the variable on the

photographs were made by the method of Argelander, the value of a "step" being approximately 0·05 mag.

Date.	G.M.T. h m	Photographic Observations.	
1900 Dec. 22	16 18	d1v, v12e	fair.
1901 Feb. 11	11 30	dov, v15e	good.
28	10 17	d7±v	bad.
Mar. 28	10 18	d12±v	poor.
Apr. 14	8 50	d15v	good.
Dec. 18	15 02	d12v4e	,,
1902 Mar. 4	10 25	d7v9e	,,
5	10 25	dov, v15±e	,,
Dec. 23	15 10	d12±v	bad.
29	14 46	d7v, v7e	fair.
1903 Mar. 3	10 45	dov, v15±e	good.
1904 Jan. 19	13 39	d10±v	poor.

The comparison star *d* is B.D. 43° 613 (9·4 mag.). The rough place of *c* for 1855 is R.A.=2^h 50^m 00^s, Decl.=+43° 19′, and that of *e* (about 10½ mag.) is R.A.=2^h 50^m 15^s, Decl.=+43° 34′. On the photographs *c* and *d* are equal in brightness, but visually the latter is a full half magnitude brighter than the former. Some of the photographic observations suggest a short periodicity, but the visual observations which follow negative this.

Date.	G.M.T.* h m	Visual Observations.	Wt.
1925 Jan. 4	18 04	d4v, v10c	5
5	21 02	d10v, v8c	5
6	21 22	d12v, v5c	5
7	22 41	d8v, v7c	3
8	22 45	d12v, v5c	3
9	20 12	d8v, v6c	5
11	20 07	d7v7c	5
15	18 57	d9v5c	3
17	18 05	d11v4c	3
22	21 03	d8v6c Cloud ?	1
23	19 14	d8v, v6c	5
26	21 49	d10v4c	2
27	21 09	d9v4c	4
28	18 43	d9v5c	5
30	23 05	d7v6c	2
31	18 20	d12v, v2c	4

* From midnight.

Date.	G.M.T.* h m	Visual Observations.	Wt.
1925 Feb. 1	18 52	d10v4c	2
2	19 26	d13v, v1c	5
6	19 00	d15±v, v=c, v15±e	5
9	19 50	d14v, v0c, v15e	5
13	20 40	c4v, v12e	5
14	20 48	c3v, v12e	3
18	19 30	d14v, v0c, v14e	5
19	20 09	d13v, v1c	4
23	19 44	d15v, c2v, v14e	5
27	20 12	d14v, v0c, v14e	5
Mar. 2	19 46	d14v, c0v, v14e	3
6	19 50	d12v, v1c	3
8	19 47	d14v, v1c	5
10	19 57	d14v, v0c	3
14	19 42	d12v, v2c	4

Only one observation is given for each night, but frequently the variable was observed at intervals during several hours. No certain change, however, was ever observed on any one night, so that a very short period is improbable. The observed visual range of variation amounts to about three-quarters of a magnitude, assuming a step value of 0·05 mag. The star decreased in brightness from January 4 to a minimum about February 13, after which there was a slight increase in brightness. Some of the minor fluctuations suggested by the observations also appear to be real. Having regard to both the photographic and the visual observations, it is probable that the star is an irregular variable.

Polvarth, St. Mawes :
1925 *March* 27.

* From midnight.